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THE

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OF

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22. The Australian Medical Journal, Melbourne: Wilson and Mackinnon. London: H. Baillière.

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23. The American Journal of the Medical Sciences. Edited by Isaac Hays, M.D. Philadelphia: Blanchard and Lea.
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33. The New Orleans Journal of Medicine. Trübner.
34. The Western Journal of Medicine. Indianapolis, Ind., U.S.A.: C. D. Cazenove, London.

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37. Journal de Chimie Médicale, de Pharmacie, de Toxicologie, et Revue denouvelles scientifiques, nationales et étrangeres, &c. Paris: Labé.
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57. Montpellier Médical Journal mensuel de Médicine. Montpellier: Boehm et Fils.

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59. Archives de Médecine Navale. Paris: Baillière.

60. Mémoires et Bulletin de l'Académie Royale de Médecine de Belgique, Brussels.

BELGIUM.

61. Annales D'Oculistique. Brussels.

62. Annales et Bulletin de la Société de Médecine de Gand.

GERMANY.

63. Zeitschrift für rationelle Medicin. Herausgegeben Von Dr. J. Henle und Dr. C. v. Pfeufer. Heidelberg and Leipzig: C. F. Winter.

64. Vierteljahrschrift für die praktische Heilkunde, herausgegeben von der medicinischen Facultät in Prag. Prague: Karn André.

65. Canstatt's Jahresbericht über die Fortschritte der gesammten Medicin in allen Ländern. Redigirt Von Pr. Scherer, Pr. Virchow, und Dr. Eisenmann. Würzburg: Stahel.

66. Wochenblatt der Zeitschrift der k. Gesellschaft der Aerzte in Wein (Beilage zu den Jahrbüchern). Redigirt von ℌ. Duchek, C. Langer, ℌ. Schauenstein. Leipzig: Hinrichs.

67. Deutsches ℌrchiv. für Klinische Medicin. Erlangen: Th. Blaesing.

68. Jahrbuch für Kinderheilkunde und Physische Erziehung. Leipzig: B. G. Trübner.

PRUSSIA.

69. Archiv für pathologische Anatomie und Physiologie, &c. Herausgegeben von R. Virchow. Berlin: G. Reimer.

70. Allgemeine Zeitschrift für Psychiatrie und psychisch-gerichtliche Medicin. Herausgegeben von Damerow, Flemming, Roller durch Heinrich Laehr. Berlin: Hirschwald.

71. Berliner Klinische Wochenschrift. Berlin: Hirschwald.

72. Archiv für Klinische Chirurgie Herausgegeben von Dr. B. von Langenbeck. Berlin: Hirschwald.

73. Monatsschrift für Geburstkunde und Frauenkrankheiten. Berlin: Hirschwald.

HOLLAND.

74. Archiv für die Holländischen Beiträge zur Natur-und Heilkunde, Herausgegeben von F. C. Donders, Utrecht, und W. Berlin, Amsterdam, Utrecht: C. Van Der Post.

NORWAY.

75. Norsk Magazin, for Lægevidenskaben. Udgivet af det medicinske Selskab i Christiania. Redigeret af W. Boeck. A. W. Münster. Lund: Voss. Christiania: Feilberg and Landmark.

SWEDEN.

76. Hygiea, Medicinsk och Farmaceutisk Manads-skrift. Stockholm.

DENMARK.

77. Bibliothek for Læger. Udgivet af Direktionen for det Classenske Literaturselskab. Redigeret af Dr. K. Brüniche. Copenhagen: Reitzels.

ITALY.

78. Bulletino delle Scienze Mediche. Publicato per cura della Società Medico-Chirurgica di Bologna.

79. Giornale Veneto di Scienze Mediche.

80. Lo Sperimentale Giornale Critico di Medicina e Chirurgia per servire ai Bisogni dell 'Arte Salutare. Direttore Prof. C. C. M. Bufalini. Florence.

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ART. I.—*Stone in the Bladder—Lithotomy in the Infant and the Child—Crushing the Stone in the Adult and Old, with the application of Lithotomy to each, in certain cases, practically considered.* By RICHARD G. BUTCHER, M.R.I.A.; M.D. (*honoris causa*) of the University of Dublin; University Lecturer on Operative Surgery; and Surgeon to Sir Patrick Dun's Hospital; late Senior Surgeon to Mercer's Hospital, and Lecturer on Clinical Surgery; Honorary M.D. of the College of Physicians of Philadelphia; Examiner on Operative and Practical Surgery in the University of Dublin; Ex-President of the Royal College of Surgeons in Ireland; Fellow and Licentiate of that Body, and for many years President of its Surgical Court of Examiners; likewise Examiner on Anatomy and Physiology in the College for many years.

I. LITHOTOMY IN THE INFANT AND THE CHILD.

II. CRUSHING THE STONE IN THE ADULT AND OLD, WITH THE APPLICATION OF LITHOTOMY TO EACH IN CERTAIN CASES.

IN my work on "Operative Surgery," published in 1865, I have considered at some length the operation of lithotomy in the infant and the child, and illustrated that subject by several cases, and a

beautifully coloured plate, exhibiting numerous calculi from the size of a few grains, up to that of several drachms, safely extracted by the lateral method of operation. Adhering strictly to the rules which I then laid down, within the past five years I have frequently operated at this tender age, and with a like success. Though the object of the present paper is chiefly to convey my views with regard to crushing the stone in the adult and old, and the suitableness of sometimes adopting lithotomy in preference, I am inclined, before proceeding, again to draw the attention of my readers to the lateral operation of lithotomy in the infant and the child—having cut a child some days since in Sir P. Dun's Hospital, it will do admirably for illustration, more particularly as I consider this descriptive demonstration the type of practical teaching. Christopher Buller, aged seven years and three months, was admitted, under my care, into Sir P. Dun's Hospital, October 16, 1869, with stone in the bladder—this I ascertained by sounding him a few days before. The sufferings of the patient for nearly a year before were sometimes excessive, particularly during the latter months, and this compelled the woman who had charge of the child at last to bring him to hospital. His history and complaints at once, in my mind, pointed to stone in the bladder. He could not for a long time before retain his water at night, and was always blamed for wetting the bed; this symptom had been looked upon as the consequence of carelessness; by day he was often seized with the most urgent desire to make water, which frequently would flow away before his trousers could be opened—the pain and distress after making it occasionally doubled the child up, so that he would crouch upon the ground, pressing the lower part of his belly; at other times, when the stream was flowing it would suddenly cease, and not continue again until change of position; latterly the child was for ever fiddling with his penis and dragging the prepuce; during the last month, occasionally on the cessation of the stream, a few drops of blood would pass away, and sometimes with the straining of the last drops of water prolapsus of the rectum would occur—thus the chain of rational symptoms was complete, and the diagnosis next practically confirmed by the introduction of the sound.

The operation was performed after the following manner, October 20, 1869, at 10 o'clock a.m. Early in the morning, a little warm water was thrown into the rectum, so as to empty the bowel, and the urine was retained in the bladder for two hours previous to the

above time. The child was placed upon the operating table, and brought under the influence of chloroform. I then passed the staff and struck the stone. Compression was made upon the penis to prevent the water flowing off. Each hand and ankle were then bound together, and the patient brought down to the end of the operating table, upon which the buttocks steadily rested, immediately sustained by a blanket, covered with a sheet neatly turned in, on a line with the edge of the table, and secured by a couple of laps of bandage to prevent slipping. In the act of tying up the child, the rectum discharged its contents, and slightly prolapsed; the part was cleaned, and the gut restored. I may here add, it is a custom of mine immediately before applying the knife, both in the old and young, to introduce my finger into the rectum, and thus solicit the evacuation of any fluids or solids which may be there, and so secure the safety of the bowel. The staff was taken by an assistant standing on the left side of the patient, and held well up to the pubis; an assistant on either side separated the limbs, and held them wide apart, the sole of each foot resting firmly on the table, so as to insure the proper axis of the pelvis. Bringing myself to a convenient height by going down upon the right knee, feeling diligently with the index finger of the left hand, the staff could be discovered traversing the membraneous portion of the urethra, I then laid the knife on, about three-quarters of an inch in front of the rectum, probably a line or two less, and immediately to the left of the raphe, carrying it with a determined weight backwards and outwards for fully an inch and a half, midway between the anus and ischium, but nearer to the former, thus dividing skin, superficial facia, and much fat. Again the knife was carried to the front of the wound, and struck at once into the groove of the staff, at the anterior part of the membraneous portion of the urethra, the instrument was moved gently from side to side, to make certain it was fairly lodged, then slightly lateralized, and passed backwards and upwards, dividing the membraneous portion of the urethra. The point of the knife was then, as it were, brought away from the groove of the staff, its blade holding the same amount of lateralization, and made to cut its way from this deep position backwards and outwards, accurately in the line and axis of the first incision to its posterior extremity. The sharp-pointed knife was then laid aside, and the narrow blunt-pointed instrument substituted, to complete the careful and accurate section of the prostatic portion of the urethra. The staff being rigidly maintained

in the same position, there was no difficulty in inserting the blunt-pointed knife in its groove; a double movement and concentration of action was now brought into requisition. The blunt-pointed knife was held up well to the groove in the staff, while the index finger of my left hand lay in the acute angle between its back and the groove in the staff, and thus the knife was pushed backwards and upwards, dividing the rudimentary prostate and neck of the bladder, the finger and knife went into the bladder together—some of the water gushed out, but some was retained by my finger blocking the opening—the staff was withdrawn, and the knife laid aside. I next introduced the gorget, its concave surface resting partly on the side and back of my finger nearly to the tip, then with half a turn it glided into the bladder, when my finger was withdrawn, and the forceps, with its closed blades, and in its narrowed axis, passed along it, the hand of the operator being in the prone position; the gorget was then laid aside, and I raised myself from the kneeling position; the instrument was rotated by half a turn, the hand being made supine. The stone, though felt, could not be readily seized, owing to the size of the instrument; a smaller one was passed in after a similar manner to the first, and on opening its blades and striking it gently the stone fell into the chops of the forceps, and was readily extracted by an up and down movement of the instrument as it rested upon the index finger of my left hand acting as a fulcrum beneath. By these combined and gentle movements the stone was extracted with the least possible force, and with comparatively little violence to the wounded parts. The stone was composed of lithic acid, and about the size of a small walnut. Finally, the finger was passed into the bladder and swept all round it, to make certain that no second stone was there. The patient was next unbound, and the *canule a chemise*, well oiled, introduced into the bladder, and retained by suitable tapes. He was then laid on his back in bed, a folded blanket under him, his knees tied together, with a small soft pad between, and the thighs slightly flexed, by a pillow placed beneath the hams; thus the wound was protected from pressure, and a ready escape afforded for the water from the bladder. Quickly the child awakened from the chloroform, and was quite unconscious of what had been done.

3 p.m.—No pain; urine passing freely through the *canule*.

21st.—The child slept the entire night free from pain, water passing abundantly through the *canule*. 3 p.m.—Urine flowing

freely by the instrument—cut the retaining tapes, so as to leave it at liberty.

22nd, 9 a.m.—Urine flowing copiously through the wound, the canule having slipped out of its own accord.

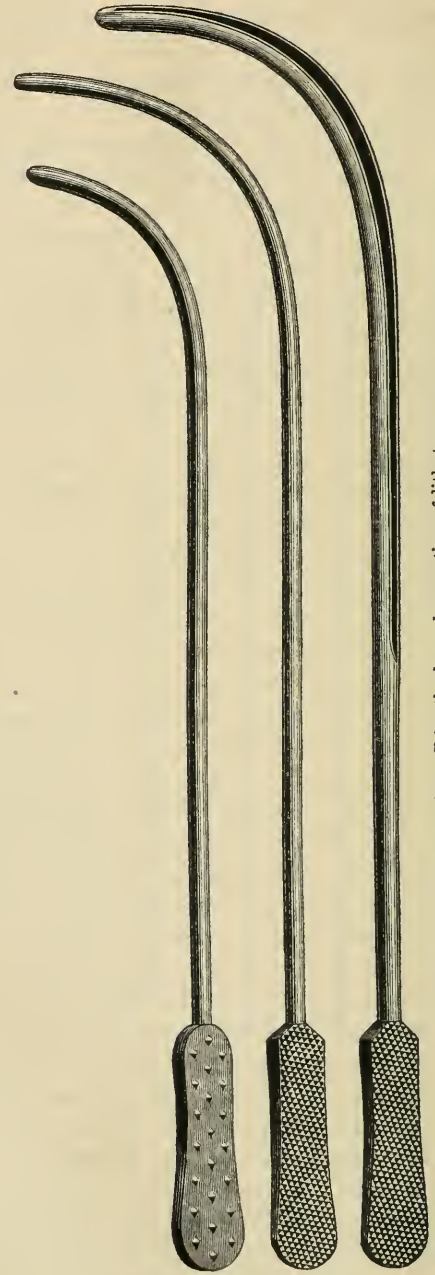
29th.—Not an unpleasant symptom since the operation, and now for the first time some urine by the penis (ninth day after the operation). Wound rapidly healing.

Nov. 5th.—Nearly the entire of the urine passing by the penis, only a few drops by the wound.

9th.—This day the water has ceased to come by the wound, which is healed up in all its deep parts, and nearly on the surface. The child has full power to retain the water, to restrain it in its flow, or to pass it with much force, and without the slightest degree of pain of any kind.

18th.—Wound healed, and child perfectly restored to health; no more pain. All the functions of the parts risked in the operation perfectly distinct. Child dismissed cured in a few days after.

I have laid particular stress upon the various steps of the operation, as I advocate its performance, yet there are some points which I wish to repeat and dwell upon. The sounding of the patient in the first instance requires a few words. It should be ascertained whether the child had passed water recently; if not, all is well; but if so, the child should be placed recumbent, brought under the influence of chloroform, and then two to four ounces (according to age) of tepid water injected. The little sound should be then warmed, oiled, and gently, lightly passed into the bladder. To facilitate this result, the instrument should not fill the urethra, but glide easily along it; but other advantages flow from the instrument not being retained tightly in its route; its point can be elevated, depressed, moved to either side—in short, made to explore the bladder with freedom, and with a certainty that never can be obtained if the urethral grip be close, biting, and constricted. The instrument most effective in detecting the stone is one somewhat short in the sweep, and with rather a short curve; so constructed, it can readily be moved in all directions with accuracy and precision. When the presumptive symptoms of stone are present, and strongly marked, the surgeon should be tenacious of his opinion, and not lightly put aside the case because he has failed on one or two occasions in finding the stone. No; the child should be placed in bed, all irritation subdued by tepid baths and suitable medicine; and after a few days the examination may again be made,



Butcher's staff for the lateral operation of lithotomy.

Size—Five-sixth scale for child.

both in the recumbent and upright position, the bladder being, of course, in a state of repletion. If the stone be not detected while the patient is lying down, then a hollow silver instrument should be passed, and the position changed to that of standing up; the urine should then be permitted to flow slowly off, and very probably the calculus will strike the instrument, and give satisfactory evidence of its presence. I have laid stress upon passing the staff before the patient is tied up, and for these reasons:—In the first place, it is easier to do so, and safer, when the limbs are relaxed; and secondly, any unexpected difficulty may unsteady the hand of the unpractised operator. The educated touch of the trained surgeon may reject this advice, but I would caution the young surgeon not to do so, for too much force may be communicated to the staff, and if so, it may very readily be passed through the upper wall of the urethra in front of the bladder, or, again, very readily behind that viscus, between it and the rectum. The tissues comprising the urethra at this tender age are so feeble in their cohesion that they will readily yield to violence; and, again, the angle to the bladder, at this period of life, is so acute that greater facility is afforded to the instrument in going astray. It is well that a couple of ounces of water should be retained in the bladder previous to the staff being introduced; it facilitates the detection of the stone; and the stone should invariably be struck previous to cutting the patient—not only because the stone, if a very small one, might have escaped from the bladder, but because this evidence not only proclaims the presence of the stone, but also *that the guide to it is direct and certain, the instrument has not gone astray.* Now as to the form of the staff: It is essential that, beyond its curve, it should be of considerable length, otherwise, when fairly held up to the pubis, there will not be a sufficiency of it in the bladder, owing to the long neck and pelvic position of the organ. As to the arrangement of the groove in the staff, it should begin well in front of the commencement of the curve, on its convex surface, so as to be at its full depth, where the surgeon strikes for it at the beginning of the membraneous portion of the urethra. I prefer the groove on the convex surface of the instrument to that on the side, subjected to this modification; *that one side be a little lower than the other, so that when the instrument is introduced, the side next to the right hand of the operator be a little less than a line lower than the opposite edge of the groove, which condition should maintain to within three-quarters of an inch of the end of the instrument, when it should gradually come up to a level with*

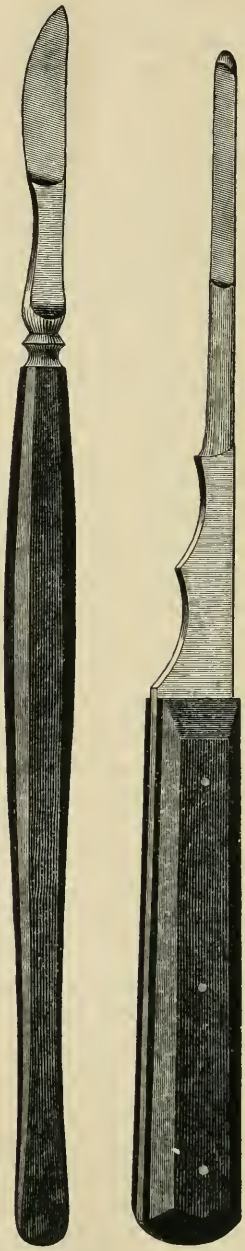
the opposite side (see woodcut); such an elevation guards against any slipping of the knife when within the bladder. This arrangement facilitates the lateralization of the knife to the required extent, without changing the staff from its proper position, held up to the arch of the pubis, and it is in direct opposition to that reprehensible practice of trying to rest the staff upon the stone, and partially turning the groove to the incised side, so as to lead the operator to it; this latter manœuvre greatly endangers the rectum; it also facilitates the slipping of the knife, and its wandering away from the director; and it may readily entail more extensive division of the neck of the bladder posteriorly than requisite, or at all safe. One word more with regard to the way in which the staff should be held —it is a word of caution to the assistant, that in his anxiety to see the wound, and the steps of the operation, he should not bend over the patient, and so depress his hand, thus changing altogether the position of the staff, and bringing the extremity of its curve out of the bladder. If so, unfortunately, the knife falls short of it, and the operator may fail in reaching the viscus, and even not be able to rectify the error into which he has been led, so I would warn the surgeon, before he completes his last incision, to look well to the position of the staff.

Now, as to the first incision, there is no use in beginning it too far forwards, for no additional space can be obtained by doing so for the extraction of the stone. The close proximity of the osseous walls on either side prohibit such a sequence; but the incision should go well back, and nearer to the rectum than the ischium, and considerably beyond the former. By so doing commensurate space is secured for the travel of the stone, and a direct outlet is provided for the dribbling of urine from the bladder.

Next comes the important step of opening the urethra. The knife should enter and strike the staff at the commencement of the membraneous portion of the urethra; the index finger of the left hand will discover the instrument and the part, and the scalpel should penetrate a couple of lines behind the first incision. The knife being lodged in the staff it should be moved quietly from side to side so as to make certain of its presence there; the instrument should then be slightly lateralized, as admitted by the construction of the staff, and so pressed steadily backwards, dividing the membraneous portion of the urethra, even to the rudimentary prostate and neck of the bladder; the hand should then be depressed, of course, bringing down the knife with it, and

so the instrument withdrawn, thus dividing the intervening parts from the termination of the membraneous portion of the urethra to the posterior extremity of the first incision, creating a great triangular gap; the apex at the prostate, the base at the perineum, direct throughout.

I cannot lay too much stress on the necessity of freely opening the entire of the membraneous portion of the urethra by the one and continuous stroke of the knife. This should be the surgeon's aim, for if the knife be introduced again and again with the intention of clearing the staff, the difficulties of completing the operation are greatly increased; the urethra is wounded and notched in several parts; shreds of it may hang into the groove of the staff, and impediments so offered to the blunt-pointed knife, so that it cannot travel freely or with security to the division of the prostate and neck of the bladder. But graver consequences may still follow this imperfect division of the membraneous portion of the urethra. It may be so injured by the repeated wounds that the remaining connecting tissues may fail to resist the efforts essential to the completion of the operation, and give way, and so the surgeon may thus be foiled in reaching the bladder. The membraneous portion being fairly divided throughout, the blunt-pointed bistuory is to be introduced into the grooves of the staff, and immediately the index finger of the left hand into the angle between it and the staff. The right hand presses the blunt point firmly to the staff, and at the same time onwards towards the bladder. While the index finger of the left hand is in contact with and pressing it in the same direction, both arrive at the rudimentary prostate, the onward movement being still continued, the back of the knife resting in the pulp of the finger, the sharp projecting edge slightly lateralized makes sufficient division, and both enter the neck of the bladder. The wrist of the right hand now is slightly lowered, and pressure downwards and to the left side gently made, with the point of the finger resting on the bistuory to the extent deemed necessary. The blunt knife and the finger being within the neck of the bladder, the blunt instrument may be laid aside; and now, and not until now, can the staff be taken out with safety. Upon the finger is then introduced the blunt gorget in the manner already described, and upon it the forceps. It is much safer, in the young or old, to conduct the forceps by the gorget to its destination than upon the finger; first, because it will travel through a smaller space; secondly, and

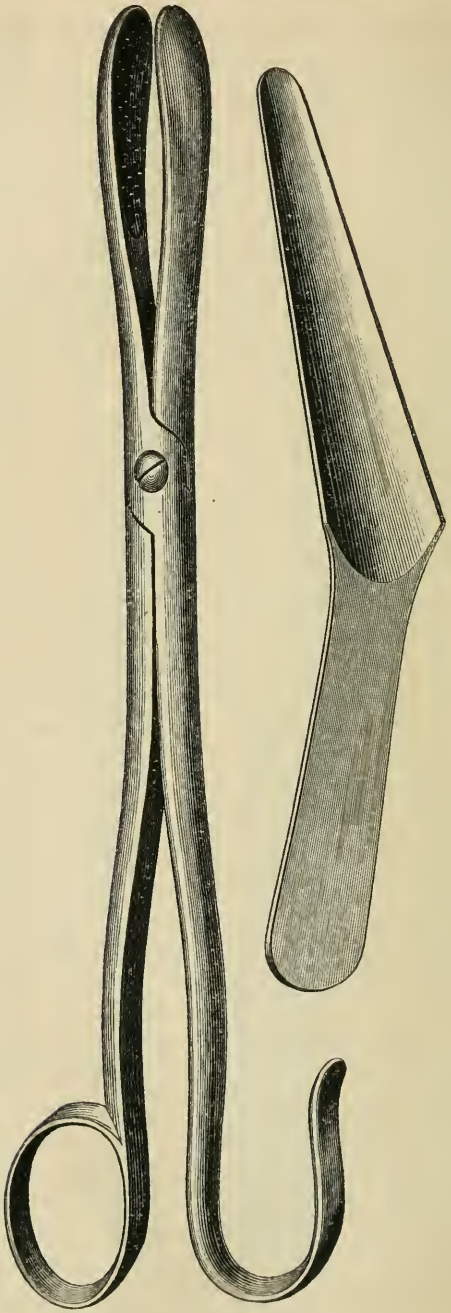


Size—Seven-eighth scale for child.

most important, the liability of the instrument's slipping aside and unnecessarily separating parts, even to the passing between the bladder and rectum, is done away with and guarded against. The forceps passed along the finger seems more showy to lookers-on; but greater security is insured to the patient by conducting the instrument through the hollow of the gorget; therefore the latter must be received and considered most practical.

I have depicted here the sharp knife which I use for the external incision, and likewise the blunt-pointed instrument for the division of the prostate and neck of the bladder. Both are represented in their form and shape for children. The sharp knife is very slightly, in the least possible degree, cut off at its point so as to make it glide along the staff more evenly. A word with regard to the formation of the blunt instrument. It should be long and narrow in its blade, quite straight, not curved like Cooper's hernia knife. I have had it constructed for myself, *with the point blunted for the eighth of an inch*, not more, not as Cooper's hernia knife, for half an inch at least. I lay great stress upon this point, because I have found, when the knife was blunted too far from the point, in gliding along the staff, *the hinder portion of the blunt part being out of the groove, hitched against parts that required to be divided, and so created difficulty in keeping the instrument in its proper track, and sometimes even tilted it from its position;* therefore I always use the instrument with little more than a button point, about the eighth of an inch long; and the cutting edge need not be so extensive as figured in Cooper's bistuory. The form of the knife I prefer and use is accurately represented in the woodcut. The gorget, made of steel, should be slightly hollowed, not wider than the index finger, or even so wide, somewhat longer, and slightly tapering towards its extremity, its edges nicely rounded off, and its handle of the same metal, set at an obtuse angle with the shaft.

The forceps being within the bladder, the gorget or forceps' director may now be taken away, and search made for the stone. Every effort should be made to seize or secure it in the most favourable axis, and the index finger of the left hand should be brought into requisition to assist this project, as illustrated in the case which I have described. When the calculus is caught, too much pressure should not be exerted upon it lest it may be broken up into fragments, a result not to be desired in ordinary cases. To prohibit such a consequence a little slide may rest upon



Size—Three-quarter scale for child.

one of the handles, and when the stone is caught it may be passed down, and so prevent any closer approximation of the blades; I have forceps so constructed by Savigney. In the delivery of the stone the index finger of the left hand should be always used as a fulcrum upon which to lever the forceps, and so materially facilitate its extraction, and that, too, with the least possible amount of contusion and injury. Such can be accomplished, even though the stone be large. The advantages of this mode of practice is adverted to in several of the cases which I have recorded in my work on "Operative Surgery," and clearly in the case which I have just recited recently operated upon.

As to the form of the forceps, it should be long and narrow in its blades, each blade very gently curved and rounded off at its margins, roughened on the inside to secure their grip, and they should be firmly riveted together, so as to prevent, by pressure through the leverage of the long handles, the approximation of the long blades, a casualty which would endanger the coats of the bladder (see Woodcut). Curved forceps may also be at hand, but they will seldom be required at this period of life. The *canule a chemise* I have had represented of its full size, for the young subject, and so it should be prepared for use. The slight circumferential groove in the outer plate of the canula, commencing half an inch above the aperture in its extremity, permits the fine linen to be fastened there in its position, with very little increase of bulk or volume. The tapes connected to the opposite end effectually retain the instrument in the bladder, when attached to a band, which should be made firm around the patient's loins; the tapes ought likewise to be carried beneath and around each thigh, and then tied. Care must be taken that the instrument is not thrust in too far; *its extremity should only rest within the neck of the bladder*, and then no uneasiness can be occasioned by its presence, and all its salutary influence may be obtained. The foregoing case marks plainly its time for removal.

In early life the parts situated in the complicated region of the perineum are so different in density, size, and position from that which they are found to present in the adult, and again in the old subject, that they truly require quite separate consideration. Again, the axis and osseous limits of the contents and space must ever be remembered in these distinct epochs of life.

In infancy and childhood the several parts concerned in the operative procedure of lithotomy holds very different relationship

Full size for child.

to each other, as well as to the important viscera in the vicinity from what they do in adult life, and again in the old subject. At this tender age the pelvis and osseous supports for the lower extremities is but imperfectly developed; therefore the parts which are destined to be within its protection at maturity are but as yet floating viscera, mostly contained within the abdominal cavity. The straits of the pelvis at this early period are quite different also, peculiarities inherent in the infantile pelvis. In the child the superior aspect looks more directly forwards, and the infant more directly backwards than they do after adolescence. Changes in the curves of the spinal column cannot be permitted to offer a solution; the pelvic axis are independent altogether of its influence. When regulated by normal nutrition, growth, and development, as many of the peculiarities of childhood return again to the old man, so do we find true apparent analogies to exist; the superior aperture at one time directed forwards, as in the infant, whilst the lower has an inclination backwards. We cannot now trace to any intrusive altèration in the bony walls, but must attribute it to the senile changes in the curvatures of the spine, and allied to the flexion of the hip and knee-joint, so habitually resorted to for repose in the aged creature. There is one difficulty which presents itself in the conformation of the bones—the measurement of the inferior outlet of the pelvis—that has been found to pertain to the adult skeleton, and does not apply to infancy and childhood. The variableness of this measurement has often given anxiety, difficulty, and failure too, to the lithotomist. On the authority of two of the most distinguished surgeons of the French school, the question is immutable. Dupuytren has examined twenty-three subjects, and found the distance intermediate between the tuberosities of the ischia to vary from two inches to three and a half inches; while Velpeau affirms he measured forty subjects, and observed in one case these processes to be but an inch and three quarters asunder, while in another they were four inches apart.

TABLES *showing some of the varieties in those parts concerned in the Operation of Lithotomy in the Child, the Adult, and the Old Man.*

Axes of the Pelvis, contrasted in the Child, the Adult, and Old Age		
Child	Adult	Old Age
The superior aperture looks much more directly forward than in the adult. Axis of inferior aperture much more backwards than after puberty.	Axis of superior aperture marked by a line from the vicinity of the umbilicus downwards to the os cocyx. Axis of inferior aperture passes upwards and slightly backwards through the mid-space between the tuberosities of the ischia to the promontary of the sacrum.	The superior aperture looks again more directly forwards, like the child. Axis of inferior aperture inclines backwards, as explained, more considerably than in the adult.
Conditions of the Rectum contrasted in the Child, the Adult, and Old Age		
Child	Adult	Old Age
Peritoneum very low—on rectum—at birth within an inch of the anus, even up to five, a very trifling interval after this, distance increases *pari passu*, with the development of the inferior fundus of the bladder.	Peritoneum covering of rectum, or *cul de sac* of peritoneum, about three inches and a half from the anus, when bladder in state of repletion. Varieties are described when the serous membrane extended to within two inches of the anus, though the bladder distended.	The prostate being generally enlarged, sustains the peritoneum, and as it were lifts it up from the anterior wall of the bowel below.
The rectum presents in its course a very faintly marked curve forwards—indeed almost straight.	The course of the rectum in its upper thirds correspond to the curves of the sacrum, while inferiorly it turns backwards as it were around the front of the cocyx; therefore concave above, convex inferiorly.	Rectum increased in length; sometimes inflected from side to side, and cavity generally dilated.
No dilatation of the rectum above the sphincters in the child.	Shape of rectum somewhat cylindrical; slightly dilated below, and well marked constriction where grasped by the sphincters.	In old age rectum often greatly enlarged, and cylindrical form gone; distended so as to embrace in its anterior wall the prostate and vesiculæ, and assuming more of a pyramidal form, the base below.

A wide range of information is conveyed to the surgeon on examination through the rectum by the finger, in the adult and old subject, as contrasted with that in the infant and the child. By

well-educated touch, on the finger being passed into the bowel in the adult and old, the position of the bulb of the urethra, its membraneous portion, the prostate gland, the vesiculæ seminales, may all be determined both in health and disease, the bulb being about an inch above the anus, and half an inch in front of the rectum; the membraneous portion of the urethra about an inch and a half from the anus, and three-quarters of an inch in front of the rectum; the prostate gland from two inches to two and a half inches above the anus, lying on the anterior wall of the bowel, and separated from it only by a line's breadth of dense cellular tissue. The close proximity of those parts affords at once an explanation of how the surgeon may derive valuable assistance by passing his finger up the rectum, and directing a catheter in its passage onwards to the bladder; how he can readily detect the amount of disease in the prostate or vesiculæ seminales; how he may be enabled, when sounding a patient, to tilt a calculus upwards and forwards when the "*bas fond*" of the bladder is inordinately developed, and thus make its presence ring by percussion against the steel instrument. The proximity of these parts likewise exhibits how readily the bladder may be tapped either above or through the prostate, and how, by introduction of the finger, all these important parts in front may be strained and so steadied as to allow, with the greatest safety, a direct passage to be made to the bladder, somewhat analogous to the median operation; while, at the same time, it will account for several sympathetic affections which may emanate from or be transmitted to this important region.

In connexion with the operation of lithotomy, the condition of the membraneous and prostatic portions of the urethra in the child, and the position which they hold to surrounding parts, must ever be remembered as contrasted with their arrangement in more advanced life. The non-developed condition of the prostate, the vertical axis of the prostatic portion of the urethra, owing to the bladder being chiefly an abdominal viscus, take, in a remarkable way, from it that support and steadiness which is enjoined by the alterations effected in adult life, the bladder then becoming a pelvic organ, the prostate encasing the commencement of the canal, each held and bound together by more firm layers and reflexions of fascia. Again, the walls of the urethra throughout its entire course are more muscular, thickened, and resisting in the aged. The cohesion between the several parts of the canal in the child is very feeble, and readily yields to violence.

Conditions of the Bladder contrasted in the Child, the Adult, and the Old Man.

Child	Adult	Old Man
Bladder narrow at neck, tapering into the urethra, the only part in the pelvis. In the fetus, when distended, it presents somewhat the appearance of a cylinder contracted at either end; soon after birth the upper fundus becomes rounder, and then it acquires the pyriform shape or flask-like. Its vertical axis greatly exceeds its transverse and antero-posterior; even when empty the greater portion of it is in the abdomen. From this form it slowly changes until up to six, when the bladder is of an oval form, both extremities being nearly equal, and very little of it rising above the pubis.	Bladder buried down in pelvis; when empty, triangular in form; base below, but not closely applied to the rectum when expanded, so as to contain half a pint or more of fluid; its figure is somewhat oval, its vertical axis considerably the greater; the transverse and antero-posterior being nearly equal. The lower end of this ovoid rests upon the rectum far more closely than in its undistended state.	Bladder more contracted; lower in the pelvis. At this period "*bas fond*" greatly developed; and when prostate enlarged, may not be easily emptied of urine, originating the first alterations and deleterious consequences of ammonial urine.
Axis of bladder very variable, owing to its being principally contained in the abdomen, and subjected to move in obedience to the abdominal muscles. During childhood the axis of the bladder appears, in the *dead subject*, to run from before backwards, nearly horizontally, because the distended bladder, no longer supported by the abdominal muscles, turns forwards over the pubis; but in the *living child*, when the recti are forcibly contracted, the line in question becomes nearly vertical.	When the bladder in the adult is over-distended, it is considerably increased in every diameter; it is reversed also in its shape; the largest extremity of the oval is now superior, occupying the hypogastric region, which it renders prominent and tense.	Bladder sometimes dilated into a pouch behind and below the level of its neck, forming even a permanent reservoir—one in which calculi are not unfrequently contained.

CRUSHING THE STONE—LITHOTRITY.

I shall now detail several cases in which I successfully crushed the stone, performed lithotrity, some of them surrounded with many difficulties, and yet, by this conservative method, with vigilant care, brought to a satisfactory and happy result. No surgeon will hesitate about the applicability of the operation of lithotrity in suitable cases, but yet there are cases in which the surgeon's best judgment will be tested in giving the preference to it over lithotomy. The surgeon will often find it difficult to be firm in supporting his views as to the propriety of adopting the latter measure,

so thoroughly does the weight of public opinion and the urgent entreaty of friends contend for the operation without cutting. When the patient comes within the range of youth and manhood, when as yet no changes, the result of morbid action or of age, are impressed upon the bladder, prostate, or urethra, the stone may be crushed with every prospect of success, with that certainty, I would say, of cure, which must, in my opinion, render any other operative measure not only unwarrantable but highly censurable. In the old subject, too, as a general rule, the stone may be crushed with safety, and with a good result; but, as I will presently describe, there are cases in advanced age where the knife offers the best chances of success and recovery. If the case is suitable in other respects, the constituents of the stone offer no obstacle to its being broken up, be it hard or soft, large or small, that is while ranging within a certain scale, say from that of a bean to a middle-sized walnut; it sharply breaks, or it crumbles down, no matter how close its cohesion, under the quick application of force, or the gradual pressure of the screw, as applied by the beautiful lithotrites of the present day. Without in the least wishing to disparage the French instruments, as constructed by Charrière, Leur, and others, in my mind nothing can exceed the perfection to which Weiss, Coxeter, and Mathews have brought these instruments, the former in his adjustment of the screw and stop, the latter in the application of the rack and pinion construction. Of course, I leave out of consideration here those cases to which lithotrity is not applicable, and where the calculus is so large as nearly to fill the contracted bladder—for in those instances the knife alone is to be depended on, and affords the only chance of relief—while the mode of operation must be adapted to each special case—here the surgeon's judgment must determine whether the lateral operation of Cheselden, or the bi-lateral of Dupuytren, Allerton's form of the median, as advocated by the Italian surgeons, or Civiale's medio-bilateral, or the recto-vesical of Hoffman and Sanson, or the supra-pubic operation, be the one selected. Whether knife or lithotrite be employed, it is essential, for the safety of the patient, that the surgeon should have an accurate knowledge of the perineum and all the parts in relation to this region. By a reference to the tables which I have laid down, it will at once be seen how this practical lesson bears weight, and the necessity for its adoption. I am induced to select many of the following cases, because operated on a long time ago, and since then frequently under observation,

proving the result of the operation to be all that could be desired, a perfect immunity from any return of the disease.

CASE I.—*Lithic Acid Stone—One inch in one axis; three-quarters of an inch across—Crushed—Perfect Recovery.*—Mr. W., aged fifty-three, applied to me on April 10th, 1864, complaining of acute pain after making water. Sometimes, though desire urgent, incapable of passing any, at others the flow would be continuous to the evacuation of the vesical contents, while at others suddenly the stream would be checked and stop; but after change of position would go on again. The pain which most arrested his attention, and urged him to come up to town, was that occasioned by riding on horseback, and still more by travelling on his outside car; being a country gentleman and accustomed to a very active life, this symptom not only distressed but alarmed him. On getting the foregoing history of his case, I suspected stone, and having sounded him, at once detected its presence. On the following morning I measured the stone, and found it to be a small one, only an inch in the longest axis, and three-quarters transversely. I inferred that the stone was very hard from the sharp click of the instrument when struck against it, and also from the acid nature of the urine. The patient was a large stout man, and the urethra was unnaturally small, as well as the entire genitals, so that, contrary to my usual practice, I had to dilate the canal by the gradual introduction of instruments for a few days, so as to enable me having passed the lithotrite, to ensure its working freely without being gripped, and also to make provision for the free passage of the fragments when the stone was broken up. Constitutional treatment, sedatives to allay pain, and the warm bath and stupes over the pubic and perineal regions, were had recourse, to allay all local irritation. A fortnight passed over in making those necessary preparations. On the 28th, assisted by Dr. Bevan, I decided on crushing the stone. The patient was placed on a narrow bed, with solid mattress support, the pelvis somewhat raised by a small hair cushion. The urine being almost healthy, the patient was directed to retain the water from two to three hours (which he was enabled to do), before the time appointed for the operation. Standing on the right side of the patient, I introduced one of Weiss's improved lithotrites, and before its blades were expanded, at once struck the stone; the instrument was then gently withdrawn, so as to leave but little more than its beak in the bladder. The female blade

was then gently passed inwards, the handle of the instrument being at the same time gently raised. The stone at once fell into position, and on pressing forwards the male blade was securely gripped. The blade was pressed forward, and the instrument screwed home. The crackling of the stone was quite audible, the fracture was sharp, and evidently the stone was very hard from the force required to disintegrate its structure. Two or three of the fragments were then taken up, and broken after a similar manner. I next introduced an instrument with short wide blades, and crushed to powder a few of the fragments and delivered in its chops a quantity of detritus. The operation was performed when, as I said before, the bladder was about one-half to two-thirds distended with water. The breaking of the stone, the manipulation of the lithotrite evoked no pain, and only a few drops of blood tinged the water that followed the withdrawal of the instrument. On the patient being visited at 4 p.m., five hours after the operation, I found him lying quite free from pain, having passed a full quantity of water and a large quantity of broken up stone. Lithic acid fragments very hard and sharp in the fracture. Ordered diluents in abundance; hip bath, and an opiate at bed-time.

29th.—Had a quiet night; passed water four times; several small fragments and detritus passed with it; one fragment impacted almost four inches from orifice, attended with great pain, just before my visit at 9 a.m. With a long-handled very narrow forceps I was enabled to seize the piece, and with great gentleness and care to extract it without the least laceration, as proved by the absence of any trace of blood. During the following four days several small pieces of stone came away, unattended with pain.

On the sixth day after the operation, May 5th, all irritation having subsided, and as no detritus came away for the two days previous, I sounded him again. Having passed in the lithotrite, I felt a portion of stone and broke it up, together with two or three fragments. This proceeding was unattended with pain, and not a trace of blood followed. Ordered warm bath and a sedative draught at bed-time; diluents freely.

May 6th.—Had a quiet night; passed water very abundantly, and small pieces of well broke up stone. At three o'clock I was sent for, and found a large fragment impacted in the fossa behind the glands. I passed in a small scoop by a little management behind the piece and prized it out; treatment as on yesterday.

On the 10th of May, the man passed water quite freely, without

the least uneasiness whatever, either before or after, or during the flow. This state of things continued, though I permitted the patient to get up and walk about his room.

On the 12th, I carefully sounded the patient, and again on the 14th; in neither instance could I detect the slightest fragments, and the absence of all pain confirmed the local examination as to the stone being entirely removed. During the past five years I have frequently seen the gentleman; he has had uninterrupted good health, and no return of the stone.

CASE II.—*Lithic Acid Stone—Size one inch and an eighth in one Axis; and an inch and a quarter across — Lithotrity—Patient aged sixty-five—Perfect recovery after the fourth Operation.*—Mr. N., aged sixty-five, consulted me in conjunction with Sir D. Corrigan, on June 28th, 1865. He was a large full man, florid complexion, and apparently in good health. He describes his case as follows: He had been suffering for two years and a half previously, with frequency in making water; pain sometimes after, and with the last drops frequently a tinge of blood. These annoyances gradually increased, so that latterly he could not take carriage or horse exercise, owing to the increase of suffering, so much so, that often he had been obliged to dismount, or get out of the carriage and walk home; sufferings produced in this way by jolting exercise, were invariably accompanied by increased quantity of blood in the urine, and during and after this state the frequency and urgency of making water were also considerably aggravated. The patient was a hard and cold man, kept a good deal within himself, so his early sufferings and annoyances were sternly borne with, and it was not until after the lapse of a long time, and when at length his pains became so acute, that he sought for relief on the above date. At this time he was unable to retain his water for more than a couple of hours at farthest, while sometimes in walking through the street, he would have to turn down the first lane or archway he met to evacuate the contents of the bladder; at times so urgent would be the impulsive power to pass it, that before his trousers could be unbuttoned his dress would be all wet. At this time, when he consulted Sir Dominic Corrigan, and myself, he had all the rational symptoms of stone, marked in an aggravated way, and it only remained now for the sound to confirm its presence in the bladder. The gentleman was placed lying upon his back on a sofa, with the pelvis well raised by a couple of pillows; the sound was readily

introduced, as the urethra was healthy and roomy throughout its entire extent. The instrument ranged to either side, and below for two or three turns without touching the stone, but upon being pressed in further, almost up to the very handle, and then gently moved to either side, the foreign body was detected at the very summit of the fundus, to which point it arrived as being made most dependent by position. The man being very large, over six feet, and the bladder of its full size, will account for the length to which the instrument had to be pressed, to obtain the required information, and more particularly, when it will be remembered, as yet the irritation had not existed long enough to contract the bladder, or morbidly spoil its coats, or even so as to effect putrefactive changes on the fluid thrown into it; so then the stone was struck audibly, sharply. In this case everything seemed so favourable to the crushing of the stone, both in Sir D. Corrigan's judgment and my own, that we determined on operating the following morning. The grounds upon which I selected lithotrity were these:—The man was in good health; he had full expulsive power over the bladder; the urethra was healthy and capacious throughout its entire extent, and the measurements of the stone were moderate, one inch and an eighth in one axis, and an inch and a quarter across; and, though believed from the analysis of the water, and from the sharp click when struck by the steel instrument, to be very dense in its cohesion, hard in its character, yet this condition was not considered sufficiently weighty to prevent the attempt being made of reducing it to fragments. On the 29th of June, at 11 a.m., in presence of and assisted by Sir D. Corrigan, I crushed the stone after the following manner:—The patient was placed upon a firm narrow bed; the pelvis well raised; water was retained in the bladder for two and a half hours previous to our coming. Standing on the right side of the patient, his limbs well apart, I introduced Weiss's improved fenestrated instrument, and detecting the locality of the stone far back in the fundus of the bladder, the blades being gently opened, by a little lateral and wavy motion from side to side, the stone fell into the chops of the instrument, the male blade was gently pressed down, and with a force sufficient to ensure its grip, until the stop was pressed down, then the instrument was slightly withdrawn away from the walls, and as it were into the cavity of the bladder, before the screw was made to work. The force required to turn the screw was very considerable at first, so hard was the stone, and it was only by forcible exertion that the teeth

of the instrument were driven into its structure, but this being once accomplished, the rending of it was quick and decided, as evidenced by the sharp crack elicited, and confirming the amount of its cohesion; four pieces were taken up consecutively and reduced to fragments, and the lithotrite was withdrawn; no blood followed the operation; the patient was seized with urgent desire to make water; a large and copious stream came off; the patient being restricted to recumbent position no portions of the stone were expelled in this convulsive effort to empty the bladder; a hot stupe was laid over the bladder and perinæum, and a sedative draught given.

Visited at 4 p.m.—Patient has not suffered any inconvenience or pain; passed water three times, and with each evacuation some fine detritus, making about half a tea-spoonful; hip bath, stupes, and an opiate at bed-time; diluents freely.

June 30th.—Had a good night; passed water four times, and several fragments of stone, some attended with sharp pain; a large piece became stuck about three inches from the orifice, producing considerable suffering. I extracted it with a long fine forceps. Constitutional and local treatment to be continued.

July 3rd.—Complains of sharp pain at head of penis; in fact his early symptoms aggravated after making water. I passed in a sound, and discovered some large pieces; on the following morning in presence of Sir D. Corrigan; again used the lithotrite after the same manner as at first; I succeeded in breaking up to powder three large pieces, pieces as expressed by the scale from a quarter to half an inch. I next passed in a non-fenestrated instrument, and brought away a good deal of detritus.

July 9th.—Since last report has gone on most favourably, passing a good deal of detritus and a few small fragments; pain diminished considerably after making water. The urgent desire to make it nearly gone, and also the frequent appeals to pass it diminished by more than one-half. The irritation after last operation having subsided, I introduced a sound to explore the bladder, and after some search discovered a small piece which was keeping up the irritation. I introduced the lithotrite, and after gentle search secured the piece, which was quickly broken up; a good deal of it was brought away by a smooth-bladed lithotrite, and without pain. Constitutional and local treatment to be persevered with, as after each of the former operations.

On the 10th (the following morning) the remainder of the detritus was brought away in the urine; after this quickly all

irritation subsided. The frequent calls to make water ceased, so that on the second day after he could sleep the entire night through without being disturbed, and by day his calls to make water were not unnatural. All pain had subsided in the effort to make water, and after passing it. To be certain that all was right I carefully examined, with a short-beaked sound, the bladder, but not a portion of stone could be discovered.

Now the patient was permitted to get up and walk about. After a few days he was allowed to go out and take carriage exercise, from which he returned without having expressed the least distress. I sounded him again, only to confirm my former examination; the entire stone was removed.

CASE III.—*Large Lithic Acid Stone, measured three-quarters of an inch by an inch and a-quarter—Crushed and Extracted, with perfect Recovery, after two Operations.*—Rev. Mr. J., aged forty-eight years, a peculiarly nervous creature, consulted me the first week in January, 1866. He stated that about nine months before he was attacked with severe pain in the loins, so severe as to confine him to bed. The pain was very acute a part of the time, and led to the supposition that a small stone was the cause of his sufferings and distress; as suddenly the pain ceased, and in a few days after he passed a small stone, somewhat larger than a grain of wheat, and in two days after a second, about the same size. These he presented to me, they were perfect examples of pure lithic acid stone. After the escape of these two stones he was then tormented by great uneasiness after making water and sometimes a sudden check in its flow, with great straining; on changing position it would flow on again uninterruptedly for some time, and would again be stopped, when by adopting a similar procedure it might again flow uninterruptedly to the emptying of the bladder. Generally the stream would be checked three or four times during the expulsion of the entire contents of the organ. He also passed a little blood on one or two occasions. The whole history of the case led to the inference of the presence of stone in the bladder, and after sounding the patient carefully I was satisfied of the presence of the foreign body, and certain that a stone was there. However, I sounded him at a disadvantage, there being at the time scarcely any water in the bladder. Not content with this examination, on the following morning I again sounded the patient, the bladder being distended, and audibly struck the stone, a very sharp click

being elicited. On measurement it was somewhat more than three-quarters of an inch in one direction, and fully an inch and a-quarter in another. The gentleman had to return to the country to make arrangements as to his professional business, and he did not again come under my care until the 26th of the following month, February. On this day, the bladder being half full of water, I gently passed in a sound, and distinctly struck the stone. While in the country all the gentleman's sufferings were augmented, and he ardently pressed to get relief from his pain. Ordered a hip bath, a gentle aperient, and a sedative draught at night. On the next morning visit on the 28th, assisted by Dr. Bevan, I proceeded to crush the stone. The gentleman being placed lying towards the edge of the bed on a hard mattress, with the pelvis well raised by the introduction of a pillow beneath, I introduced Weiss's improved fenestrated lithotrite with a gentle steady movement into the bladder, the penis being steadily drawn forwards as it were upon the instrument with the left hand, while the right sustained the lithotrite in its direct course to the opening in the triangular ligament, when the hand sustaining the instrument was suddenly depressed, so tilting its point through this narrowed part, and so directing its onward course with the greatest gentleness and least possible obstruction into the bladder, and so in accordance with the strict anatomical arrangements of the region. The instrument being within the bladder, it was gently passed forwards, and next in succession to either side, but without touching the stone. Then the lithotrite was slightly rotated, its point depressed towards the left side and downwards, when an audible click against the stone was felt and heard. I gently withdrew the instrument, opened its jaws, and after a slight waving movement of it from side to side, the stone dropped into its grip. On closing the male side and fastening the slot, I screwed home the instrument, which required much force to break the stone. However, it soon gave way, and with a very clear and distinct sound of its disintegration. I next opened the instrument and searched for the larger fragments, reducing three considerable ones to minute portions. I then cautiously closed the instrument, and withdrew it. The patient, though extremely nervous, yet admitted I did not hurt him. After the removal of the lithotrite there was an uncontrollable impulsive movement to empty the bladder; the urine gushed out at first slightly tinged with blood, and so in the recumbent position the patient was allowed to pass the water; when nearly done I

restricted him in the act, and so he was enabled to retain some. I then gently introduced a catheter, and threw in from three to four ounces of warm water, to allay irritability and float, as it were, the broken fragments from the coats of the sensitive viscus. The pillow was removed from beneath the hips, and the patient directed to lie quietly on his back, a large pillow being placed beneath the hams, so as to flex the thighs. In half an hour after a full opiate was given. Visited at 4 p.m.; had some sleep; no pain; pulse a little accelerated; skin hot. Ordered a diaphoretic mixture, and diluents freely. He made water, and passed several fragments of lithic acid stone, varying in size from a grain of wheat to three times that bulk, besides a quantity of detritus. Ordered to be constantly stuped over pubis, genital organs, and perineum; opiate at night.

March 1st, 9 a.m.—Had a quiet night's refreshing sleep; pulse soft and 78; skin moist; passed water three times in the night and once this morning; several large pieces of stone came away, lithic acid, with some coating of phosphatic deposits, together with detritus; to continue diaphoretic mixture, stupes, opiate. 6 p.m.—Passed two very large pieces of stone; a third was stuck in the urethra, about four inches from the orifice; the pain was so great that I was sent for. With a long narrow forceps, after a little gentle handling, I got out safely the piece, without laceration; opiate at night, and constant stupes.

March 2nd.—A large quantity of detritus passed, and three small very irregular sharp pieces.

On the 4th the bladder was so quiet, so free from irritation, that I again introduced the fenestrated lithotrite, and broke up two considerable pieces without pain or suffering, or any appearance of blood. Three hours later he passed a copious supply of water, being far better able to retain it, and with the full stream a considerable quantity of detritus. During the next two days he passed occasionally some detritus. On the 7th I was agreeably made aware that he only passed water once from the night previous, that it flowed in a copious stream, without the slightest straining, uneasiness, or check of any kind, and with not a particle of detritus in it. The gentleman was up now and walking about, and so he continued for two days, when I submitted him to a searching examination, lest a fragment might have been left behind. Directions were given that the urine should be retained from two to three hours. When I passed a scoop lithotrite with a slender

stem, and searched the bladder most carefully in all its parts, the construction of the shaft of the instrument enabled me to move it in all directions without any grip in the urethra, but I could detect no particle of stone. This examination was conducted without the patient expressing the least pain, so completely had the bladder recovered its healthy condition, all source of irritation being removed. A few days later the patient returned to the country, and at once resumed his duties, and I have good reason to know that now he enjoys perfect health.

CASE IV.—*Lithotrity on a Patient aged sixty-seven years—Large Phosphatic Stone, two inches in one Axis, and one inch across—Entirely removed by Three Operations.*—Rev. Mr. ——, aged sixty-seven, consulted me in September, 1866, for stone in his bladder. His health was greatly broken down from months of agony which he endured. He had long been suffering from an affection of his kidneys as well as that of the bladder. When I saw him he could scarcely retain a wine glass of water, and the pain he endured on passing it could not be described; a few drops of blood immediately following the expulsion of the last drops—ten, fifteen, eighteen times in the night, and nearly as often by day—he was urged to make the effort to pass it. I placed the patient in bed, and put him under the most soothing kind of treatment—baths, sedatives, diluents, &c.—rest in the horizontal position, with hips well raised by pillows. Gradually the increased irritation and distress which he laboured under after a long journey from the country subsided under this lenient, mild treatment, and after a few days I was enabled to throw a little warm water into the bladder, and gently introduce a sound; this I accomplished so as to satisfy myself that a large stone was there. The urine was putrid, alkaline, loaded with mucous, accompanied with a highly offensive ammonial smell. Strict constitutional and local means were enforced to allay this terrible irritation—opiates, acids, buchu, belladonna (most valuable), with constant stuping over the vesical and perenial regions, and occasional hip baths were had recourse to, and soon with salutary effect. The bladder lost a good deal of its irritability, the urine became clearer, the mucous diminished; the terrible watchfulness consequent upon the repeated demands to make water gradually lessened; he had better nights, more sleep, and he was able to consume considerably more nutriment. The advantage of raising the pelvis, and thus allowing the stone to lie in the upper fundus of the bladder, a point

which I have laid great stress on before, was very marked in this case; whenever the pillows were taken away his sufferings were produced in a very aggravated form. Now came the question, as we gained a period of repose, what was to be done? no doubt the kidneys were engaged, but yet the presence of the stone entailed such misery, that the patient implored that it should be taken away, his life was so wretched, his sufferings so terrible; surgery prompted to the same issue, but the practicability of crushing the stone was surrounded with difficulties, owing to the morbidly sensitive condition of all the parts concerned; still the prostate was not enlarged. In consultation with Professor Bevan, taking every particular of the case into consideration, we came to the conclusion that lithotrity was alone applicable, and I decided on crushing the stone October 1st, 1866. 11 o'clock a.m.—The patient was placed on the right side of the bed, supported steadily on a hard cushion, the pelvis was considerably elevated, more than I usually direct, and the head resting horizontally with the body. Chloroform was gradually administered, and its soothing and quiescent influence rapidly produced; the limbs being held well apart, standing on the right side of the patient I introduced a catheter and injected four ounces of water at the temperature of 100° into the bladder. The catheter immediately revealed the site of the stone; it was withdrawn, pressure with the index finger and thumb of the left hand being made on the penis to prevent the escape of the fluid; I next passed in Weiss's fenestrated lithotrite, when it readily came upon the stone far back, where it was expected to be found, owing to the well raised position of the pelvis maintained for some days. On separating the blades of the instrument, after two or three gentle sweeps, the stone fell within its grip, and was immediately steadied, and the slot drawn down; the force necessary to screw home the male blade was but small, the stone readily crumbling to pieces, proving its phosphatic nature; by very gentle efforts, consecutively, four pieces were caught, and at once broken up into debris. So soft was the stone that when the instrument was withdrawn it was impacted throughout the intervals of the teeth. The patient awakened from the chloroform, and was much surprised to hear that the stone had been broken up, as he had not felt any part of the proceeding. After the removal of the instrument a good deal of water rushed out, slightly tinged with blood. Ordered warm stupes over part, a full opiate, and some warm wine and water, hot jars to feet and loins. Visited at 4 p.m.,

found the patient quiet, free from any marked pain; he had passed more water at a time than he had done for weeks, and with less distress; some powder and small fragments of tripple phosphate were discharged; took beef-tea freely; pelvis kept elevated, and with great benefit; for the three following days, he greatly improved, the character of the water was altered very much for the better, the distress and urgency in making it very much diminished; so that on the 6th, assisted by Dr. Bevan, I again proceeded to crush the stone, having gone through the same arrangements in placing the patient as on the first occasion, bringing him under the influence of chloroform, injecting the bladder with warm water, &c. I as cautiously as before introduced the lithotrite, and readily seized a considerable piece of the stone, which by gentle force pulverized, so on a second and a third piece; this breaking was effected with the fenestrated instrument; I then withdrew it and passed in a fine scoop lithotrite, and pulverized several pieces. No efforts were made by the patient to shrink from the manipulation which I carried on, so admirably did the chloroform act, and by such quiescence I was enabled to apply the greatest gentleness, at the same time to act with the utmost precision on the fragments. After the instrument was removed the water that passed was very lightly tinged with blood; position, opiates, stupes, warm stimulants, diluents, as before. When visited at 5 o'clock p.m., he was comparatively easy, and had passed a fair quantity of water, and nearly two tea-spoonfuls of the broken up stone in powder and small bits. At 7 o'clock I was called suddenly to him, as he could not pass any water, and having a great straining and desire to do so. I gently passed down a full-sized silver catheter, and was obstructed just at the membraneous portion of the urethra by a solid body. I felt this at once to be a portion of stone impacted, and too large to get on. I endeavoured by gentle pressure to push it back, but could not do so by the amount of force which I considered it safe to employ, so I passed down a full-sized catheter with an open end and rested it against the foreign body; I then applied a syringe to the end of the instrument, and sent down a gush of water with considerable force, this, after one or two efforts, was effectual in liberating it and carrying it in its current back into the bladder. The force of the column of water was effective, because acting in two ways—by its presence it lifted away as it were the sides of the urethra from the irregular sharp edges of the stone, while the weight and force of the centre of the column drove the

stone before it back into the bladder. The removal of the stone was speedily followed by the bladder emptying itself, and so giving relief; a large quantity of the stone came away. After two days, irritation having greatly subsided, and as particles of the stone ceased to be discharged, I again, on the 8th, applied the lithotrite, adopting all the precautions before mentioned, and with like success; the ready finding of the stone, its easy disintegration; the patient quickly recovered from the influence of the chloroform, and passed water without any discolouration by blood; a large quantity of sand was discharged with it. Ordered a full warm opiate, stupes, diluents freely, and nourishing broths. Visited at 5 p.m., when he expressed himself as feeling more comfortable than he had been for months, particularly remarking after making water he had no pain. Visited on following morning, 9th, when I found he had a good night, passing water only three times, and not attended with pain. I introduced a sound and could not discover any stone. This examination was unattended by much inconvenience, and I was as much rejoiced as the patient to find the entire had been taken away, after the third operation the amount of stone filling nearly an ounce bottle. Visited at 5 p.m., when I found the patient hot and restless; pulse rapid; countenance flushed; urine in very small quantity. Ordered a diaphoretic mixture and diluents; hot stupes over pubis and femoral regions. Seen again at 10 p.m.—condition worse; more excited; no urine passed, so introduced a silver catheter with great gentleness, but no urine in the bladder; complete suppression; dry cupped over the loins, diuretics; prescribed stupes, &c.; gin punch freely. Found the patient worse on the following morning; not a drop of urine secreted; introduced a catheter, but not a tea-spoonful of fluid in the bladder. He lingered on, gradually getting stupid until early the following morning, when he died of uremic poisoning. I was much distressed having failed by every means to obtain a *post mortem* examination in this most interesting case.

CASE V.—*Tripple Phosphate of Ammonia and Magnesia Stone, one inch and a quarter in one axis, and over three-quarters of an inch in another—Lithotrity—Patient fifty-eight years—Recovery after fourth Operation.*—M. N., aged fifty-eight years, came to consult me in November, 1867. He stated that, though an Irishman, he had been resident in Jamaica for the last twenty years; that he returned from abroad three years since invalided. He suffered when

away from terrible ague, and for the last twelve months from great pain in the bladder, and in the effort to expel and retain the urine. During the latter six months his sufferings have been greatly augmented, and he was in the habit of frequently passing a gum elastic catheter, as he thought, and was directed, to relieve the bladder. Before coming to me he was treated for ague and cystitis, and without the least relief. On the 16th of the above date he came to my study, trembling all over, and with urgent desire to pass water; he got a vessel to do so, and I observed his efforts, the jerking squirt, straining, change of position—again some flow, and so on to the finish—about four ounces being discharged, highly coloured, from being slightly tinged with blood. After the expulsion of the water his sufferings were excessive; he writhed on the sofa, and trembled in the most violent way. A warm opiate with brandy was immediately given, which soon quieted the paroxysm. On closer inquiry I found that latterly he always felt this torture after passing water, with acute pain shooting to the end of the penis. On consideration of the entire case, as he described his sufferings, and as they were revealed to me in his efforts to empty the bladder, I came to the conclusion that he had a stone in the bladder. I ordered him to take a hip bath at night, and put him on large doses of belladonna, Brandishe's solution, and sedatives—these acted most soothingly, and with excellent effect, so that on the 19th, four days after the scene of misery which I have described, he came to my study again. I then gently sounded him, and detected a stone, which registered on the scale of the instrument one inch and a quarter in one direction, and over three-quarters of an inch in another direction. I decided on crushing the stone. The man was so broken up in health that I was certain he had not power to repair any wound. The bladder was able to expel its contents, and the urethra was roomy and capacious—circumstances of much moment in coming to a conclusion as to the safest operative procedure to be adopted. The gentleman was kept on the sedative treatment, and restricted to quiet for a few days, and on the 22nd, assisted by Mr. William Colles, I proceeded to crush the stone at 11 a.m. The patient was placed for the operation after the manner which I described in the former cases. He would not take chloroform, therefore I had some trouble both in injecting the bladder and forcing the required quantity into it (about four ounces), as he could not retain latterly more than two to three ounces.

However, the water was delivered, and the lithotrite gently and

steadily introduced, not, however, without much wincing on the part of the patient. He was then allowed a few moments to rest, and search made for the stone. Now, though the pelvis was well elevated, and the bladder fairly distended, yet the stone did not fall as well back as I have been in the habit of finding it on these precautions being taken. However, one or two gentle sweeps with the instrument discovered it, and on cautiously opening its blades, and lightly shaking it, the stone fell within its grip, was secured, and rapidly broken up. The stone was caught across, according to the measurement before revealed, and it readily crumbled down, proving the stone to be of phosphatic origin—five large pieces were taken up in succession, and crushed to powder. I then withdrew the instrument, a good deal of the crushed stone came away in its blades, the contained water squirted out after it, tinged with blood, and some detritus with it. Ordered a full warm opiate draught—hot jars to feet—stupes over the pubis and perineal regions. 5 p.m.—Has suffered some pain—was inclined to rigor, but cut short by warm sedative draught, and hot jars to loins. A good deal of water passed, with some detritus, but no fragments—to continue diluents—stupes—opiates. I was now for the first time informed that when abroad he indulged largely in the use of opium, so I had to increase my measure, and also ordered him three grains of quinine every fourth hour in pill. 8 p.m.—Was suffering much pain from an impacted piece of stone, two inches from orifice of urethra; this I easily extracted with a fine forceps after a little time. I injected about three ounces of warm water, with a little watery extract of opium, and withdrew the catheter—the fluid was retained, and he experienced much comfort from its presence. Opium and quinine to be continued, also diluents freely, stupes, &c.

Nov. 21st.—Called suddenly at 8 a.m., the patient suffering from retention, and such great agony. I discovered a piece of the stone entangled behind the membranous portion of the urethra, very firmly. I passed down the catheter with a cut off end, as on other occasions, forced down through it by a syringe a heavy column of water, which on being repeated gradually freed the stone, and ultimately forced it back into the bladder. I then let off the water which had been contained in the bladder, and injected three ounces at 100° in temperature, with a weak solution of watery extract of opium—attended after a short time with the happiest result, the greatest relief from pain and all irritability. Stupes to be continued. and opium and quinine every fourth hour.

3 p.m.—Free from pain since last visit; has passed water frequently, with much gravel and sediment—bathed in sweat after an ague attack—increased the quinine to five grains every third hour—sedatives, diluents—urine as before.

9 p.m.—Very quiet since last visit, and has passed several large fragments of the stone—to continue sedatives, quinine, belladonna, &c.

Nov. 22nd.—Had a quiet night—several hours of undisturbed sleep—pain very much diminished after making water—passed a very large amount of sediment and small fragments, within the three days more than a large table spoonful—very little mucous in the urine—clear and better in every way—irritability so much subdued, decided on the following morning, 23rd, on breaking up the rest of the stone. At 9 a.m., assisted by Mr. William Colles, we prepared the patient, and placed him in the same attitude as in the first instance. The lithotrite was cautiously introduced, five ounces of warm water being previously injected, and a portion of stone seized and crushed. Again, a piece considerably larger, marking on the scale three-quarters of an inch, was reduced to powder, very little force being required, the stone was so soft. Four more pieces were then severally reduced to powder, and the instrument withdrawn. A gush of water followed, and a large quantity of the recently broken up stone. For four following days little to report, save the safe passage of fragments and detritus; the same constitutional treatment pursued, and during the entire time no rigor. On the 28th, so little irritation that I introduced the scoop lithotrite, and broke up several small pieces, and delivered in its blades a large quantity. Continued all treatment constitutional and local. On Dec. 1st, again introduced the scoop lithotrite, when I detected only one little piece, the powder after the last crushing being safely expelled up to this date. I broke this remaining fragment, and brought it away in the lithotrite.

On the following morning, 2nd, found he slept the entire night, being only once awakened to make water—all pain after making it gone. The bladder retains now six ounces of fluid before it is called upon to contract; the urine is free from urea or pus. On the 3rd, I sounded the patient carefully, but could not detect the least fragment left behind. In a week after this time all symptoms of irritation gone. The patient cured.

CASE VI.—*Large Phosphatic Stone, with Lithic Acid Centre—*

Crushed and Removed successfully after three Operations—Size one inch and a half by an inch.—Mr. K., aged seventy years, consulted me in July, 1868, for stone in his bladder. He had been suffering for several months most acutely from all the paroxysms and distresses consequent upon that affection. The nature of the case was recognized and made out months before the above date by the able surgeon under whose care he placed himself in the country, Dr. Joseph Fergusson, and all that could be done to allay the local irritation and suffering was fully carried out, but yet the foreign body was in the bladder, and fresh and repeated outbursts of mischief quickly came on, and ultimately urged him to seek for its removal.

The day after the above date he was seen by Dr. Fergusson and myself, when we entered into every particular of his case, and we decided on sounding him the following morning, having ordered a hip bath and a gentle sedative at bed-time.

I was sorry to learn in the history of the case that Mr. K. had been a delicate man for years, independent of the local affection, having had a very weak stomach and feeble heart. These were dispiriting complications with the great suffering and distress consequent upon stone in its advanced stage. On the next morning, July 27, sounded the patient, and at once struck the stone audibly. The stone was measured one inch, and a little more than a quarter in one direction, and an inch in another.

On examination by the rectum, the prostate was not found much enlarged, the left lobe slightly, but yet nothing worth considering; there was no tenderness on pressure. Both Dr. Fergusson and myself considered the case favourable for breaking up the stone, and indeed to this proceeding alone the patient would give his consent.

A few days were allowed to elapse, and means taken as much as possible to lessen local irritation, and prop up the weakened system.

On the 29th, assisted by Dr. Fergusson, I proceeded to break up the stone. The patient was arranged on a firm bed, in the manner described in some of the foregoing cases, with the pelvis well raised, a few ounces of warm water (five, at temp. 100°) were thrown into the bladder, which with difficulty was got to retain this quantity, so great was the irritability of the organ. The fenestrated lithotrite was next introduced, and the stone quickly discovered and seized; it was a soft stone, and crumbled down rapidly beneath the screw, four of the larger pieces were taken up in succession and reduced to

powder. The instrument was then withdrawn, a gush of water quickly followed, tinged slightly with blood, and containing some detritus in it. The pain was not great during this proceeding; ordered stupes constantly over the pubis and perineum, diluents and sedatives; removed the wet sheet and pillows beneath him, but elevated the pelvis again on a fresh one, even more than during the operation, so as to relieve the irritated neck of the bladder from the pressure of the sharp edges of the crushed stone. Visited at 4 p.m.—Quiet; passed water and some fragments.

30th.—Had a fair amount of sleep—passed a large quantity of water, all tinge of blood gone—some detritus and small pieces of phosphatic stone—sedatives, stupes—nutriment and wine to be continued.

Three days having passed over, and but little local irritation, I again introduced the lithotrite, and broke three large pieces, a good deal of which I delivered in the scoop instrument.

6th August.—A large quantity of stone now passed, fully a dessert spoonful, with great relief from pain and suffering; can retain water in much larger quantity, and expel it with far greater force and precision. Was sent for suddenly, owing to a piece becoming impacted about two inches from the orifice, and which I extracted with long fine forceps. Owing to the irritability of the stomach and the weak heart I had much to contend against in this embarrassing case; yet, by watchfulness and care, he was relieved, after a third application of the lithotrite, from the entire stone, the bladder expelling part of the crushed material. On the 13th I sounded the patient most carefully, and again on the 15th, and could not detect any portion left behind. On confirming this conclusion, as arrived at from actual examination, the local symptoms bore testimony, as the pain of making water had entirely ceased. The pain at the end of the penis was entirely gone. The power of retaining the water was greatly restored, and the impulsive desire to make it was now absent. The patient was permitted to get up and walk about, and though relieved in every way from local annoyance, yet the patient's spirits remained greatly depressed; he continued, however, well, from any vesical annoyance.

After about a fortnight's time this gentleman went to the country, when his heart symptoms gradually became more embarrassing, and he lived for a few months. I was glad to hear from his brother-in-law, a medical man, that from the hour the patient

left Dublin until the day of his death he never complained of the least pain in making water, or of any of those symptoms which characterized his sufferings before I crushed the stone, and successfully removed it from the bladder.

CASE VII.—*Tripple Phosphate Stone—Size two inches and an eighth in one direction, and somewhat more than one inch and three quarters in another—Successfully Crushed and Removed from a Gentleman, aged seventy-eight years, after eleven Operations—Perfectly Cured.*—Mr. W., aged seventy-eight, consulted me on July 17, 1869, owing to supposed calculus in the bladder. He was suffering severely from urinary annoyances for over four years. He was sounded on three occasions, and by different surgeons; one pronouncing the presence of a stone, others not. However, on the above date, I was requested to see the gentleman. On the following morning I sounded him with great care and considerable trouble. The bladder was so irritable as not to contain, of its own accord, an ounce of water. However, I threw in about three ounces, and with difficulty retained it while I introduced a sound. In a few seconds I discovered the stone, and from the solid resistance to the instrument I was apprized it was a large one. I quietly withdrew the instrument, satisfied by the accuracy of the exploration. The history of the case was well given as one of stone in the bladder. The patient dated his sufferings, of a decided character, as far back as four years and a half, when, after a walk, or being driven on an outside car, he felt considerable pain in making water, and straining, when generally some blood followed the effort. Gradually from that time he had pain after passing water. Sometimes the stream was arrested in its flow, when, by change of position, it would again pass on. Latterly the repeated calls to make water were more urgent, and attended with increased pain, so that he felt a perfect inability to ride or drive. The urine became gradually thick and loaded, and most offensive after resting for a couple of hours; and now he suffered the most agonizing pain at the neck of the bladder after making water, and continued to the extremity of the penis. The patient's general health, when I saw him, was completely broken up from continued suffering, loss of rest and appetite. I confined the patient to bed, and ordered such means as would allay the increased local irritability—sedatives, narcotic stupes, hip baths, &c. In consultation with Mr. Adams, on the 20th July, I measured the

stone; it marked beyond the index nearly a quarter of an inch. Being gripped tightly, it measured a little more than two inches in one direction, and somewhat more than one inch and three quarters in another. The instrument was then gently withdrawn; some blood followed, and a good deal of suffering.

The patient never had any affection of his urethra during life; therefore the instrument glided through the channel without interruption. The prostate was not enlarged, neither was it tender on pressure; but the urine was most offensive, and loaded with mucous. I must say my feelings at first tended towards performing the lateral operation in this case, and at once, ridding the man of the enormous foreign body, I inclined to this view, from the healthy state of the prostate, and the fortitude with which the patient submitted to my judgment; but I inclined more particularly to this cutting operation from the success which followed my efforts in almost a similar case sometime before. However, the patient and friends were very anxious that the stone should be broken if possible; and as the urethra was healthy, and as there was no valid objection to the operation of lithotrity being performed, though large the stone, Mr. Adams agreed with me in adopting this mode of procedure. The patient was confined to bed, and continued on the same soothing line of treatment for three days, as was adopted from the first. On 23rd July, at 11 a.m., I decided on the operation, when I was assisted by Mr. Adams and Dr. Bennett. The patient was placed in bed on a firm hair mattress, with a folded hair pillow beneath the hips, so as to raise the buttocks, and so allow the stone to gravitate backwards, when water was thrown into the bladder, and the viscus partly distended. His head lay on a horizontal line with his trunk, the knees being drawn well apart. Chloroform was then administered, and the patient brought fully under its influence. The bladder, as I have before mentioned, could not be brought to retain two ounces of urine; therefore I had to distend it to what I considered a safe extent for the operation by gently injecting about six ounces of water at the temperature of 100°, the catheter being cautiously removed, and the penis compressed. Standing on the right side of the patient, I quickly introduced Weiss's improved fenestrated lithotrite down to the triangular ligament, and when steadied there depressed the handle between the legs, tilting the point upwards, and with a gentle onward motion into the bladder. The stone was readily

detected, the instrument opened with its blades wide apart, and the stone gripped. Four turns of the screw were forcibly made, when the stone broke with an audible noise. Again a large fragment was secured and broken up; another and another were acted on, and then the instrument was gently withdrawn; some blood and water followed. The chloroform was discontinued, and the patient awoke quite unconscious of what had been done, and asking when the operation was to begin. The pillow was removed from beneath the hips, a dry one not so thick introduced, and covered with a folded sheet. Half an hour later a warm anodyne was given; hot jars applied to the feet, after which the patient fell into a calm sleep.

3 p.m.—Patient free from pain; passed some water.

24th (9 a.m.)—Had a good night; passed water frequently, mixed with some blood; no fragments. Treatment, sedatives, stupes, baths.

28th.—Since last report passed several well-crushed pieces of stone, phosphatic deposit, with some layers of lithic acid. Parts so calm that I was determined to use the lithotrite again.

Assisted by Mr. Adams and Dr. Bennett, the patient was placed and chloroformed, and the bladder was injected with five ounces of warm water, and the fenestrated lithotrite introduced, several large pieces were broken up. Five times the instrument was used with good effect, and then taken away. The injected water instantly rushed out, slightly tinged with blood. Opiates, stupes, &c.

29th.—Had a good night; passed water sometimes, and a great quantity of detritus, and several small pieces of stone. Not the same urgent desire to make water, and the bladder somewhat more retentive.

August 3rd.—Since last report passed a large quantity of fragments, but for last twenty-four hours none; bladder quiet; rest fair; appetite good. Again applied the lithotrite, assisted by Mr. Adams and Dr. Bevan, the patient having been chloroformed, and the bladder injected with five ounces of water previously. Four applications of the instrument were made at this time, and with great advantage, considerable-sized pieces being seized and broken up. The patient awoke after the instrument was withdrawn, and the operation completed without being aware anything had been done to him. So he went on gradually improving, numerous pieces, and of considerable size, being passed, as well as a large quantity of detritus.

On the 10th of August, again assisted by Mr. Adams and Dr. Bevan, I used the lithotrite, previously throwing six ounces of water at 100° into the bladder, and dulling sensibility by chloroform. Several small pieces were seized and crushed, and one large fragment measuring three quarters of an inch. Owing to the quiescent condition of the patient, and the improved retentive power of the bladder, great service was rendered by this operation. With the greatest gentleness six times the instrument was applied, and crushed away portions of stone; and after its withdrawal, though water gushed out, yet no blood tinged its colour. We looked most favourably on all that had been effected by this procedure.

August 11th.—Had an excellent night's rest; drank freely of barley water, and seltzer water and sherry, and broth. There was a large and healthy secretion of urine passed, with increased power by the bladder, and attended by the expulsion of nearly a tea-spoonful of small fragments. Again the immunity from pain now was very marked; the patient entirely lost the straining after passing the last drops of water. All these conditions were improved up to the 17th, when a new complication showed itself—inflammation of the right and left testicle. The amount of stone in fragments and detritus passed up to this time and collected, irrespective of what could not be saved when the patient's bowels were moved, weighed more than three drachms (exactly three drachms and five grains). None passed for two days previous to the above date, and there was no suffering before making water, and but little after, no impulsive desire to pass it; and all this time the character of the water was quite changed; all pus and mucous had ceased to be present. I should mention that the patient has been for the last fortnight taking a mixture of hops, buchu, and dilute nitro-muriatic acid, and opiates steadily administered. For several days after the above date the inflamed testicles were very troublesome, attended with the most severe darting pains through them and down the thighs. The treatment I submitted him to, constitutionally and locally, was the following:—Lint soaked in a very strong solution of watery extract of opium and belladonna, and laid over the testicles, well supported on a pad placed between the legs, and a towel passing beneath across from one thigh to the other, together with relays of warm stupes outside the narcotic dressing, all covered with oiled silk to retain both heat and moisture; while a pill was given containing a grain of calomel,

half a grain of James's powder, a grain of quinine, and a grain of opium every fourth hour; also, a full opiate at night, with morphia suppositories (quarter grain in each) two or three times in the twenty-four hours to relieve uneasiness about the rectum. Under this treatment the inflammation quietly subsided, the acute sufferings and symptoms ceased. Now the pills were stopped, and the soothing local means continued. Thus the patient rapidly improved up to the 23rd, when he had but little to complain of. I then sounded him, but could not detect any stone, as there was not sufficient water in the bladder. I again examined him on the 25th with a like result. The patient felt so well that he put on his clothes and walked into the drawing-room wonderfully strong, and without assistance.

25th.—Passed a good night, only disturbed once to make water; character of the water much improved, and in a full quantity; testicles nearly restored to normal size. The right one, however, is still a little fuller than it ought to be; parts well supported with a suspensory bandage.

27th.—Much as on yesterday; went out to drive in an open carriage, and the night after did not feel altogether so well; he had more frequent necessity for making water, and pain both when passing it and after; stupes, sedatives, &c.

29th.—Did not feel so comfortable about the bladder; great frequency in making water, and on this morning and yesterday both mucous and a little pus passed in the fluid, with the alkaline smell that so strongly characterized it in the first instance. I suspected from his return of suffering, and the changed character of the water, that some portion of stone must be still in the bladder, and on sounding him carefully confirmed my suspicion by audibly striking a considerable sized piece. Having sounded the patient so carefully on two occasions before he was allowed to go out and drive, and not being able to discover the presence of any foreign body, I was led to the conclusion that this portion of stone now discovered had got into some pouch in the bladder, remained there quiescent for several days, until by the jolting of the carriage it was thrown again into the vesical cavity, producing its unmistakable train of symptoms as evidenced at first. I at once decided on the application of the lithotrite. Confining the patient to bed the day before, on August 30th, at 11 a.m., assisted by Dr. Adams and Dr. Bevan, the patient was placed under the influence of chloroform, and the bladder injected as on former occasions. Then I introduced

the fenestrated lithotrite and seized and crushed a very large stone, measuring more than three-quarters of an inch, and next applied the instrument to several fragments, five applications of the instrument in all. A large quantity of detritus was expelled with the water on the lithotrite being removed. The man quickly recovered from the chloroform, having suffered no pain, being quite unconscious that anything had been done to him; an opiate in an hour's time, stupes, &c. 4 o'clock p.m.—Free from pain; no return of uneasiness in the testicles, though a renewal of this troublesome complication I greatly apprehended. Repeat opiate at bed time.

September 1st.—Passed three very large fragments. September 2nd.—Passed several large fragments; quantity and character of water greatly improved; he has continued his buchu and nitro-muriatic acid mixture steadily, as well as opiates both in draughts and suppositories; pain before and after making water considerably diminished; appetite excellent, and sleeps well.

September 6th.—Again used lithotrite, crushing four good sized pieces. 3 p.m.—Several small pieces came away and some detritus.

September 7th.—No additional pieces passed; good night; free from pain, except after passing water.

September 12th.—Again introduced lithotrite, assisted by Dr. Bevan; patient under chloroform; broke up four pieces, and removed with scoop lithotrite a large quantity of small bits and detritus. Again on the 16th used the fenestrated lithotrite, assisted by Dr. Bevan; broke up many pieces and removed several small ones with the scooped instrument; I was struck by the size of one piece crushed at this operation. On the 23rd September sounded again, owing to symptoms indicative of pieces still left behind. After throwing into the bladder six ounces of warm water I discovered a large piece, and at once passed in the lithotrite and broke it up into several pieces, each again being caught in the broad shaped instrument and reduced to smaller pieces. All this procedure was carried out under the influence of chloroform, and the patient was not sensible of having been meddled with; no trace of blood after any of the past operations. 3 o'clock p.m.—Some pain along whole track of urethra; the patient passed off several large fragments, some as large as a pea, and very sharp and irregular, some of them of so great a size that it really was difficult to conceive how they could get away simply by the force of the current of the water. It must be remembered, however,

that now the tone of the bladder was greatly restored—very good, and by which he was enabled alone to expel these pieces, and also to retain a large volume of water to impel and float them away. Opiate-stupe constantly applied over the pubis.

24th.—Had sleep; passed several pieces of stone, and fully a table-spoonful of small detritus; stupes.

25th.—Had an excellent night, and looks very bright and happy; passed away several pieces more, of well broken up stone, and a good deal of detritus; altogether from this last crushing more than a dessert spoonful of fragments and detritus has been passed.

26th.—Three large pieces passed. 29th.—Out driving on yesterday, and no pain, neither after passing water. 30th.—Out driving, and no pain.

October 2nd.—Passed a lithotrite (a small one) to examine the bladder. Having previously placed the patient under chloroform, and injected six ounces of water at the temperature of 100°, assisted by Dr. Bevan, I discovered, after much search, one small piece, which I caught and crushed into powder. A good deal of detritus was removed with the scoop lithotrite; no blood from the operation.

October 3rd.—Slept well; passed a good deal of water and about a tea-spoonful of small pieces.

October 7th.—No more stone passed; no pain after making water, or at any other time; has been out walking and driving in an ordinary cab over the paved streets, yet without any uneasiness or desire to make water. I passed a small sound, and most carefully examined the bladder, without being able to detect the least fragment of stone. On the following morning, 8th of Oct., the patient was placed under chloroform by Dr. Bevan. I injected the bladder with six ounces of warm water, then introduced the scoop lithotrite and swept every part of the bladder with it, turning the instrument down behind the prostate, and at the same time passing the index finger of my left hand up the rectum so as to tilt forward any pouch of the bladder that might conceal a fragment. Yet, by the most searching examination, I could not detect the least portion of stone. Thus, after the eleventh operation, the patient was entirely freed from this enormous stone, and most successfully cured.

October 14th.—Visited the patient this day; during the past week has been out walking and driving every day, and feels relieved from every anxiety and pain; no forcing in making

water, and has gained strength and physical power in a most remarkable manner.

In the management of the foregoing cases, I have laid great stress upon the gentleness with which the lithotrite was introduced into the bladder in every instance. The practitioner must never lose sight of the fact, that it is a widely different thing to pass a straight instrument into the bladder, and making the urethra straight for the instrument, a proceeding which never can be accomplished, owing to the manner in which the membraneous portion is tied up to the pubis by ligaments before and behind, and strong reflections of fascia which render it and the prostatic portion of the canal permanently concave to the bladder. The lithotrite, warmed and oiled, should be passed down to the opening in the triangular ligament, and then the handle of the instrument depressed well between the thighs with a gentle onward movement, when it glides without opposition into the bladder, thus representing in its introduction a lever of the first order. The instrument once in the bladder must be handled with the greatest lightness, and carried to the superior fundus and back part of the bladder, when most likely it will touch the stone; then in withdrawing the male blade the calculus very often falls into the lithotrite. If it should not do so I would say, on the surgeon touching the stone he can easily determine on which side of the instrument the calculus rests; then he should turn the instrument gently to the opposite side, open the blades, and so opened throw it over the stone to the other side, and thus on closing the instrument he will find, most likely, the calculus has been caught; if not, or if any doubt as to the relationship of the stone and instrument exists, the operator should again open the blades of the instrument and cast it to the opposite side over the stone, and then probably he will be more fortunate in securing the foreign body. However, generally speaking, the slightest lateral movement of its blades will determine on which side the stone lies. In this quiet way *the instrument may be applied to the stone in the happiest manner.* Again, as reported in many of the foregoing cases, the lithotrite on entering the bladder, was for a moment held quietly, and the instrument steadily, while the male blade of the instrument was insidiously introduced so as to transverse the lower part of the bladder, the centre of the bladder, and the space beneath. After the stone fell into the jaws of the instrument it was secured and rapidly crushed. There is a movement

which I have practised in one or more of the foregoing cases with the lithotrite, and which I have also alluded to in using the forceps in lithotomy as a means of securing a grip of the stone, adverted to, by Sir B. Brodie. I shall quote his own words. If on the introduction of the lithotrite within to the lower and back part of the bladder, and on the separation of its blades, the stone does not fall into the lithotrite, "the forceps, without being moved from its situation, may be gently struck with the hand on one side, or on its anterior part, and the slight concussion thus communicated to the bladder, will probably be sufficient to dislodge the calculus and bring it within the grasp of the instrument. If it should be otherwise, the forceps being closed may be very gently and cautiously turned to one side or the other, so that the curved extremity of it may make an angle of 25° or even 30° with the vertical line of the body, then opened and passed in the direction of the rectum, in the manner already described."—(*Trans. Med. Chir. Soc.*, Vol. XXXVIII., p. 174.) The chief advantages of this proceeding is to place the instrument in a position presumed to be advantageous, *and to bring the stone to the instrument.*

On the perusal of the foregoing cases it will be perceived that I do not approve, after crushing the stone, of working away in extracting the fragments; if the urethra is free, and in that condition which would warrant lithotrity, the fragments must be left for expulsion by the contractile force of the bladder itself; it may be said, the bladder in its deranged state, so contracted, so irritated, cannot expel the detritus; if the bladder is in this state I would say cut the patient; if the bladder is in this condition that it has no power in expelling its contents, and so the broken up fragments, surely the most sceptical must admit, it is in no state to admit with impunity, without seriously resenting the irritability and violence created by the oft-repeated introduction of lithotrite, or catheter for the suction bottle, to mechanically extract those portions of stone recently broken up. When the bladder is inflamed even secreting pus, absolutely on the verge of ulceration in some patches, how can it be credited that this practice can prove salutary? No doubt, in some instances, cases seen early before all the serious consequences of stone in its advanced stage are set up; the calculus may be broken up into fine fragments, and extracted piecemeal, with good instruments and a steady hand, without much pain and with little or no harm to the patient; and yet his urethral membrane may be torn, as I have seen by this quick method and

extravasation of urine occur, and I doubt not that this method may lay the foundation for permanent stricture of the urethra. I must emphatically say I do not sanction this method in even any of the cases that will bear it; but, the large mass of cases of stone in the bladder in the adult and old, will not permit such obtrusive meddling. The irritability is so great, the changed tissues of the parts by disease are rendered so sensitive, that the surgeon will be fortunate if even, after preparatory sedative and soothing means, he can get an exploring instrument, sound, or lithotrite into the bladder; and when fortunate in doing so and making himself accurately acquainted with the nature of the case. he is glad to pause, and consider the most suitable way of removing the foreign body, if the urethra is sound, though all this terrible anxiety about the bladder, he may and possibly will decide on crushing the stone, gently using the instrument for a few applications to it; first, in its primitive size, and two or three times after to its larger fragments, and then leaving them to be cast out by the efforts of the bladder in expelling the urine; and it is very remarkable, at least so I have constantly observed, when, after the stone has been well and carefully broken up, how soon the terrible excitability subsides, the sharp edges of the broken pieces are soon smoothed down, and on the second or third day large quantities of detritus are expelled, though for the two former days scarcely a fragment had come away. The irritability being subdued and the muscular tone of the bladder developed, a larger quantity of water is retained, and a greater expulsive power is restored, and so the fragments now begin to pass with the larger and more certain continued stream.

However fortunate the surgeon may be in crushing the stone in properly selected cases, yet there are others, as I have before adverted to, where the knife offers a far better chance of success. When, for instance, the case is of a long standing, with the vesical symptoms greatly aggravated, the bladder contracted with its mucous membrane highly inflamed and even secreting pus, when the prostate is enlarged and the "*bas fond*" of the bladder, deep, always holding in it a quantity of fetid ammoniacal urine and pus, when the urethra is hard and rugged, and spoiled in its natural structure from repeated attacks of inflammation and venereal excesses. In these cases a direct passage into the bladder by the lateral method of lithotomy is the correct mode of proceeding. In a few seconds the stone is taken out, the source of irritation removed, the fetid contents of the bladder has a ready outlet. I have no fear of

dividing the enlarged prostate; if moderately done, it will in most cases then yield to force, sufficiently for the purpose of extracting the stone without passing beyond the fascial covering of the gland. I admit, in some instances, the prostate is very firm and rigid, and will not tear, then the surgeon should pass the index finger of his left hand into the opening, and on it convey down the blunt pointed knife and divide the rigid parts. Sooner than cut extensively I prefer breaking the stone than extracting it in pieces through the wound, and then washing out the bladder. If the opening through the prostate be carefully executed by limited section and forcible dilatation or rather tearing, there need not be much apprehension of serious bleeding from the prostatic plexus of veins or vessels. I shall detail a case where I removed by this careful section from a gentleman, aged seventy-eight years, the large number of twenty-seven calculi.

Lithotomy on a Man, aged seventy-eight—Twenty-seven Calculi, varying in size from a pea to a large marble, removed by the Lateral Method.—J. N. came under my observation, through the kindness of Dr. Ledwich, a short time since. The gentleman had been suffering for several years from irritation in his bladder, mucous urine, &c., and latterly with so much difficulty in making water that he came up from the country for advice. Dr. Ledwich and myself sounded him carefully, suspecting the presence of stone. The patient was placed in the recumbent position, and the sound carefully passed, and the instrument made to explore in every direction except the "*bas fond*," which could not be reached, owing to the enlarged prostate; I then passed the index finger up the rectum to lift forwards the pouched bladder, but this movement, so often successful, failed to give information as to the presence of the stone. I next withdrew the sound, and passed a hollow one, placing the patient in the erect posture and allowing the urine to flow off gradually, and succeeded in striking a stone even audibly; towards the end of the flow much blood and pus was passed, and attended with great irritability and suffering. On considering the age, pain and suffering from the least attempt at retention of water, the rugged unnatural state of the urethra, we came to the conclusion, in consultation, of the propriety of cutting him rather than crushing the stone. Assisted by Drs. Ledwich and Bevan I cut him by the lateral method, and after the manner to a certain extent which I have described for children. The patient was laid on a table; the rectum having been

emptied by a water enema early in the morning, and the urine retained. He was then brought under the influence of chloroform; I introduced the staff, and satisfied myself and those standing by of the presence of the stone; he was then tied hand and foot according to the usual method, and drawn well down to the end of the table; the knees were kept apart, and the staff was held by an assistant well up to the pubis, and the scrotum likewise; the first incision was commenced an inch and a-half in front of the anus, carried outwards and backwards, fully an inch behind the latter, and somewhat nearer to it than the ischium through the skin and fascia; I then struck the knife into the groove of the staff close up to the anterior part of the incision, passed it gently from side to side to make certain of its safe position, and divided freely back the membraneous portion of the urethra, and then freely cut outwards all the parts to the back of the first incision. I next introduced the index finger of my left hand along the staff to the prostate, and run along the groove of the staff the blunt pointed knife, following it with the index finger on its back, and pressing it downwards and backwards lightly, and thus dividing the prostate in the most measured way, and to a small extent. When a gush of mucous and urine flowed out I retained my finger in the bladder, withdrew the staff, and on the former introduced the gorget, and by it the forceps, extracting by repeated efforts the large number of twenty-seven calculi, one or two sometimes, two or three sometimes together, as they varied in size from a pea to a large marble; they were all soon removed, as I satisfied myself by introduction of my fingers. The entire time taken from the time he was chloroformed, bound up, staff introduced, cut, and twenty-seven calculi taken out, only occupied thirteen minutes, as marked by one of my students present. There was no bleeding, from the careful way in which the prostate was incised. He awoke quickly from the chloroform, and was quite unconscious that any operation had been performed. The following case is most interesting as occurring likewise in the very old subject, where calculus had existed for a long time, and all its attendant train of misery, where lithotrity had failed, and yet when in the most unpromising condition I cut him by the lateral method and extracted the stone with the most perfect success. The particulars of this case, as President of the Royal College of Surgeons, I detailed to the Surgical Society of Ireland the very evening after the operation.

Lateral Operation of Lithotomy, successfully performed for the removal of a Large Calculus, after Lithotrity had failed.—The patient was a labourer, aged sixty-eight years, and was admitted into Mercer's Hospital under my care in April, 1867. He stated that he was a sufferer from stone in the bladder for four years; throughout the greater part of this time his sufferings were excessive. Four months before his admission the stone was detected by an able surgeon in the country, and lithotrity performed upon him. A few fragments were broken off and expelled from the bladder; soon symptoms of irritation set in, and no further attempt made of breaking up the stone. A few weeks after he was sent from the country and placed under my care.

The patient suffered most intensely after the journey the day after admission to hospital; he was writhing in torture, ever trying to pass a few drops of water, and always an aggravation of suffering after the effort and the issue. The usual means for subduing inflammation and irritation of the bladder were had recourse to; the warm bath, hot stupes over the vesical region and perineum; the internal administration of sedatives and antispasmodics, together with wine and stimulants, and strong broths to sustain the wearied powers of life. I would wish to lay great stress upon the necessity of attending to position in cases similar to the present—the hips and loins of the man were raised upon pillows far higher than his chest and head, which lay on a horizontal plane, the object being to allow the stone to fall back from the irritated and most sensitive neck of the bladder, and so when the organ even was empty that its weight should be directed by gravity backwards. I cannot illustrate the force of this position more strongly than by quoting a case from my work on Operative Surgery, when for several years the patient selected it for himself as most bearable, and for such a length of time as to cripple up the lower limbs beyond the power of extension; in this state he was brought over from Wales to me for operation. The case is thus described. (*Butcher's Operative Surgery*, p. 735). George Martin, aged seven years, admitted to Mercer's Hospital, May 29th, 1860. He was a native of Wales, and brought over to be placed under my care. His father was a sailor, and gave a most graphic account of the boy's pains and sufferings. He stated that for three years and a half the boy was affected, and that for two years he was almost constantly confined to his bed. His suffering and agony was so great in the act of making water that sometimes he would rush out of the bed

screaming and tearing the prepuce and penis, and even pitch himself with the greatest violence against the wall to try to kill himself when a terrible fit of the stone would seize upon him. Latterly he was quite bed-ridden and incapable of straightening his limbs, *owing to his always assuming one position in the bed, so as to* obtain comparative relief. The boy always lay upon his back, with his buttocks and hips propped up, supported far higher than his abdomen or chest, and this was with the intention that the stone should fall back towards the superior fundus of the bladder, and away from its sensitive neck. The presence of the calculus was detected in Wales, but all operative interference deferred from time to time; at length worn out, emaciated with suffering and torture, the boy was brought over on the above date and placed in hospital. His appearance was truly pitiable; he was worn almost to a skeleton, and his haggard look of suffering, apprehension, and terror was pitiable to witness. When I first saw him he lay in the remarkable position which I have described; and then his father detailed to me those particulars which I have just mentioned. His knee-joints were stiffened, and his hips were stiffened; the legs being flexed upon the thighs, and the thighs upon the pelvis; the spine had also a slight inclination forwards, from the manner in which the head had been constantly propped up. When the boy was placed upon his feet he could not elevate his trunk or straighten his limbs, but at once placed his hands upon his knees to prevent his falling forwards. I sounded the patient, and detected readily a very large stone, which by measurement exceeded far and away any that I had before met with at this early age. I cut out the stone strictly after the manner which I have described in the early part of this paper. The calculus was of great magnitude, growing for years; in weight one ounce and a half; its size is accurately delineated in Plate 51, Fig. 7, and its appearance on section Fig. 8, (*Butcher's Operative Surgery*).

The report goes on to say—"On the 20th of June, all the urine passing by the penis and wound rapidly closing up; the limbs permit of considerably more extension. On the 25th—wound in perineum all healed, and the irritability of the bladder all gone; the little fellow can retain his water all right, and he has erections without pain; the limbs have now entirely lost their rigidity, and the boy can stand up quite straight. On the 30th—he was up and *walking* about the ward for the last two days; the bladder has regained its full tone and power both in retention and expulsion.

On this day he was dismissed cured, and left the hospital with a little fellow also cured, who was cut for the stone the same morning as himself." To come back to the important case under description. After some time the patient's sufferings were comparatively lessened, and by a perseverance in the adoption of those means which I have alluded to, with but little variation, for some days, the urgency to make water, and the rapid demands to do so, were not so oppressive; from being up some ten or twelve times in the night, and nearly as often by day, as was the case on his first admission, tranquil rest at night for a couple of hours together was insured, and probably as long intervals of repose by day; nevertheless the case was still a very extreme one, surrounded with difficulties and risk. On examination by the rectum, the prostate was discovered considerably enlarged, the lateral lobes almost in equal proportions (the right half somewhat the larger), forming a large massive tumour. I sounded the man, to make certain of the presence of the stone, in reference to which none of the equivocal symptoms were absent. The greatest gentleness was adopted in the introduction of the sound, yet it was by no means an easy matter to make it lightly travel over a very rugged urethra. The size of the instrument employed was a medium one, No. 9 of the catheter scale. On the conveyance of the instrument into the bladder extreme torture was experienced, as scarcely any water could be retained either by the efforts of the patient or by artificial supply. The bladder would not distend, therefore the range of the sound was very limited; it seemed gripped. I then passed the index finger of the left hand into the rectum, and felt the bulging of the bladder backwards, and through the coats of it and those of the rectum, readily felt the stone, and lifted it up to the sound, striking upon it with a sharp and audible click not to be mistaken, and recognizable some feet from the bed. Certain now as to the presence of the calculus, its locality, and the pathological state of the viscus itself, as well as that of the prostate and parts around, I at once removed the sound as quickly as I could, and with as much gentleness. I measured the stone, and found it to be in one axis an inch and three-quarters, and an inch in another. Considerable irritation was set up after this examination, and days of the most careful management were required to subdue it, on the principles which I have already laid down in similar disturbance, and as in relation to this case on admission to hospital. I may add, however, so tardily did the excitement subside, that I determined

on not again sounding him. From the contracted condition of the bladder, the enlarged prostate, the enfeebled state of the man from repeated suffering, now that a time of repose was obtained I considered it most advisable to remove the stone by the lateral method. The patient being prepared for the operation, the rectum being cleared out by an enema, administered the evening before, and on the morning of the operation by some warm water being thrown up; as no urine could be retained a few ounces of warm water, at the temperature of a hundred, was injected, and the bladder would not relax for the reception of more; so he was brought into the operating theatre, placed under chloroform, the staff passed, the stone struck, bound hand and foot after the method described, and brought to the end of the table; the ordinary incisions were adopted, and the bladder reached, with the knife shaped as figured in one of the woodcuts in the early part of this paper, the instrument being, of course, larger for the adult subject. I did not find it necessary to use the blunt-pointed knife; the index finger of my left hand passed along the staff, lay within the neck of the bladder, and then the staff was withdrawn, and upon the finger the blunt gorget conducted to the bladder, and by the gorget the forceps. The gorget being taken away, search was made with the forceps for the stone; but the bladder was so contracted, and the prostate so large that it could not at once be discovered, I quickly passed the index finger of my left hand up the rectum, and lifted the stone into the chops of the forceps and commenced to extract; a good deal broke away from the stone, and it slipped loose, but by the same manœuvre I instantly caught it again, and extracted a large piece, fully the size of a hazel-nut, this being the nucleus, and pronounced by Dr. Haughton to be composed of oxalate of lime. I next with the forceps and scoop removed a large quantity of considerably sized fragments, and a good deal of detritus, and then washed out the bladder, and passed my finger all round its cavity to make certain no second stone existed, and that all the fragments were removed. There was scarcely any blood lost, and but a very few moments passed in executing the operation. The staff used was such as represented in the woodcut, proportionately enlarged, and I cannot speak in too high praise of the form of the staff—to these points I have before adverted. I have mentioned I did not use the second or blunt-pointed knife in this case, because I wished to make the least possible opening in the prostate in the first instance, and having got my finger into the bladder I dilated

sufficiently. As to the advantages of the blunt-pointed knife depicted in the woodcut, they are dwelt on when speaking of lithotomy in the infant and the child, and in the deep perineum of the old its value is incalculable. The *canule a chemise* was introduced and fastened in its birth. I have mentioned the more fragile layers of the stone were broken down when grasped by the forceps. Had I not been able to extract it through the small aperture which I made into the bladder, I was prepared with a lithotrite to break up the stone rather than increase the opening in the diseased prostate and irritated structures around. Under similar circumstances I most assuredly recommend this practice as the best.

On April 3rd the report taken states patient doing well; not a bad symptom. On May the 4th the wound was healed up, and on the 18th the patient was dismissed, and with perfect power over the bladder either to retain the water, to check it in its flow, or to expel it with greater force. I could enumerate other cases to substantiate my views upon these subjects, but I consider what I have stated is sufficient, as in their recital they would only be a repetition of the facts which I have laid down exactly in a practical point of view, and as I have learned them.

The subjects considered in this paper are of the highest importance. The presence of calculus in the bladder at any period of life is surrounded with difficulty, and carries with it imminent peril to the sufferer; therefore it is that I have made the subject my special consideration for a long time, and hesitated in recording my views until I had practically noted down and learned the lessons which I now offer to my professional brethren, being well assured they will stand the test of trial, and, above all, if carried out, will afford security and cure to the unhappy creature affected with stone in the bladder, be he young or old.

Art. II.—*Surgical Reports.* By George H. Porter, F.R.C.S.I.; Surgeon in Ordinary to the Queen in Ireland; Senior Surgeon to the Meath Hospital and County of Dublin Infirmary; President of the Pathological Society of Dublin.

I.—A LARGE CYSTIC ADENOID TUMOUR OF THE FACE OF THIRTY-SIX YEARS' GROWTH—EXCISION—RECOVERY.

II.—A LARGE ENCYSTED TUMOUR OF THE NECK TREATED BY DRAINAGE AND INJECTION—LONG-CONTINUED SUPPURATION—RECOVERY.

In planning and performing operations on the face and neck, the avoidance of deformity becomes a consideration only second in importance to the actual removal of disease, and many persons have endured unpleasant and even dangerous affections for years in the apprehension that their extirpation might be followed by deformities still more unsightly. This anxiety about personal appearance will not be disputed, it is part of our nature, and therefore universal, and a due recognition of it seems peculiarly the duty of the surgeon, whose profession involves the mitigation, if not the relief, of every form and degree of human suffering. If we look to the face especially, it is exposed and therefore liable to accidental disturbance, and from its construction and uses it is perfectly impossible to apply or maintain the most effective dressings; its structures are loose and mobile in every direction, and there is a quantity of fatty substance to favour such mobility; its muscles are loose also, generally having but one fixed attachment. When to all this is added the absolute necessity for the performance of certain functions, breathing, speaking, eating, drinking, &c., &c., and the disturbance unavoidably caused thereby, the difficulty of dealing with this branch of surgery becomes but too evident, and it may be easily understood how the very best intentions and the most careful treatment have proved abortive, or, at all events, failed in affording the desired satisfaction. In addition to wounds of the face, whether caused by accident or inflicted by the knife of the surgeon, abnormal growths in this situation possess a peculiar interest. The diagnosis of their nature and immediate origin is so wrapt in difficulty and obscurity that the practical surgeon, in dealing with them, must put forth all his skill. And if what has been said of tumours of the face be true, those in

Fig. 1.

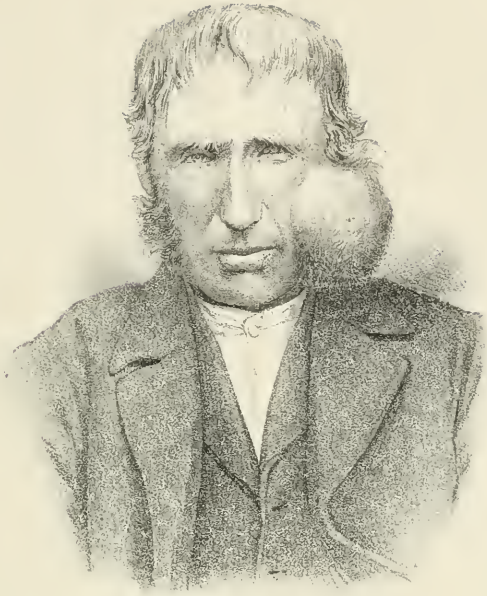
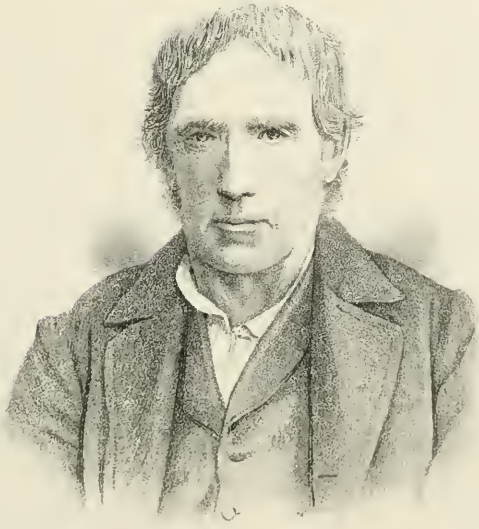


Fig. 2.



MR PORTER ON TUMOUR OF THE FACE.

forster & Cº Dublin

the neck must command even greater consideration. When we contemplate the numerous anatomical relations in this region, the great blood vessels passing to and from the thorax, along with the nerves so essential to life, it may be safely affirmed that their importance is not surpassed. The highest surgical authorities agree in the occasional trouble imposed on the surgeon to determine accurately the seat, attachments, and actual nature of a tumour in the neck. Glandular, cystic, fibroplastic, aneurismal, fatty, cancerous growths may all be met with here, and it behoves the operator to weigh carefully every point before attempting the removal of them. If lying beneath the skin, and perfectly movable, their extirpation is simple enough; but if, on the contrary, deeply attached and tightly bound down, interference with them must be anxiously considered. The two following cases, in which operative measures were successful, and each possessing features of interest, I deem worthy of being reported.

I.—*A Large Cystic Adenoid Tumour of the Face of Thirty-six Years' Growth—Excision—Recovery.*

Simon L., aged fifty-six years, a farmer, was admitted into the Meath Hospital under my care, on the 26th February, 1869, with a tumour about the size of a large orange occupying the posterior half of the left cheek, and encroaching to the extent of one inch on his neck beneath the angle of the jaw. Its superior boundary rested on a level with the malar bone, and behind it compressed the external auditory meatus into a mere perpendicular slit. On examination it was firm to the touch, and was more *uneven* than actually nodulated. The base was broad, and to a great extent fixed, the limited motion permitted being from before backwards. The same solidity seemed to pervade the entire growth, no point feeling softer than another. The skin, which was loose over it, was not discoloured in the least degree, and the swelling was nearly covered with hairs, those of the whisker being spread out. It was not translucent, as proved by testing it in a dark chamber with the magnesium light held at the side of the growth, whilst a stethoscope, placed on the opposite side, was looked through. And here allow me to recommend their powerful light as a great aid when examining whether a tumour is transparent or not. It can be conveniently produced by igniting a piece of magnesium wire fixed in a cork. He complained of no pain whatever, and said that its unsightly appearance was his only annoyance for many years;

but that for the last six months he had felt the motions of his jaw somewhat impeded whilst eating. The history of his case as given by himself was that thirty-six years previously he noticed a small hard lump, about the size of a pea, on his left cheek, just in front of his ear, and that it went on enlarging steadily ever since.

At first it was quite movable, and painless. He had frequently shown it to medical men, some of whom recommended its removal, but as he did not suffer he always declined any interference. His health was excellent, and he said that he had never been subject to any other swelling; but that this one had become so remarkable that he was followed by persons gazing at him whenever he went to a fair, or any other such public place. He had consequently made up his mind to get it taken away.

Although by no means what might be termed a movable tumour, still the great length of time it had taken to arrive at its present growth, coupled with the absence of pain, and any apparent decline of his health, led me to decide (after consultation with my colleagues) on extirpating it.

Operation.—March 10th.—The tumour having been shaved, he was placed on the operation table with his shoulders slightly raised, and his head thrown a little to the opposite side. When he was thoroughly under the influence of chloroform I made an incision from the top of the tumour to its inferior margin in the centre line, sinking the knife deeply into its structure. I then dissected up the posterior flap of skin, and separated it well from the growth backwards until I got free of its posterior limit; this was accomplished by touches of the knife, together with tearing by means of the handle. The temporal artery was laid bare in this step of the operation, but not injured. I then dissected off the anterior covering of skin, which I found more adherent, and consequently experienced greater difficulties in separating. Two or three small arterial branches bled, but were quickly secured. Having now removed the entire tegumental covering of the tumour, I proceeded to extirpate it. To the malar bone it sent a strong attachment, which I was obliged to divide with the scalpel; this done, I employed the handle of the instrument, at the same time I forcibly pulled the tumour from its bed with a vulsellum, which I had fixed deeply in its substance; some caution was here required in order not to tear or wound Steno's duct, and fortunately having got safely below the level of this canal the attachments became looser as I approached the inferior boundary,

and thus the mass was removed completely. There were five small arteries ligatured, and when the site of the growth was well sponged out, the flaps of skin were approximated by five fine pins with the twisted suture; the covering appeared at first to be superabundant, but in the dressing I laid a good thick pad of lint on the wound, which was firmly kept close by a bandage, thus preventing any accumulation of blood. The patient was then placed in bed and got an opiate draught.

Nothing of importance occurred worthy to detail during the treatment of this case from the date of the operation until he left the hospital. The dressings were of a simple character, being pledgets of lint wet with a lotion consisting of one drachm of solution of chloride of lime to eight ounces of water, kept applied with a bandage. The wound healed most kindly, and he was discharged the institution cured on the 17th of April, 1869. The prominent features of interest with which I consider this case endowed were, the great length of time it was growing, its considerable size, although almost completely located in the face, yet not connected with any enlargement of the bones. Facial tumours when non-malignant, and having no origin in the osseous foundation of the face, rarely acquire large dimensions. The operation for its removal, although not a difficult one, required caution, and the shrinking of the flaps of skin, which at the time of dressing the wound threatened to be superabundant, proved that the less integument lying over tumours the surgeon removes the better, particularly in the face, if he wishes to prevent distortion of the features by the subsequent contraction of cicatrices; had I taken away a portion of skin over this growth, in all probability, when the wound had fairly healed, it might have drawn the left angle of the mouth greatly from its normal position. The lithographs, Plate I. Figs. 1, 2, by Mr. Forster, from photographs taken before and after the operation, faithfully show the deformity produced by the tumour, and the benefit arising from its removal. The mass was weighed immediately after its excision, the weight ascertained being eight ounces and a half. Subsequently a microscopical examination was made of its structure by my late lamented colleague, Mr. Collis, who was well known to be a most able and truthful microscopist. A few days before his untimely death he kindly furnished me with the following result of his examination:—Mr. Porter's case of tumour from the side of the face—March 11th, 1869. I have carefully examined several parts of this tumour. It is cystic-adenoid. The cysts very numerous and filled with fibrous

or fibroid tissue. The texture of the tumour is throughout firm, as is to be expected from its great chronicity. There are, however, a few soft spots in which more rapid growth appears to have lately sprung up. Some of the cells in these parts are larger and much resemble cancer cells, but are readily distinguished from them by their greater refracting power, and by their greater resistance to acetic acid. They are a form of cell not uncommon in the softer or more recent portions of fibrous tumours, and are of no importance unless when they predominate over the fibrous elements. They indicate that in these portions a greater and more recent growth has taken place than in the rest of the tumours. The probabilities are against this tumour returning, but all cystic tumours are more or less treacherous in this respect.—M. H. Collis, 25, Lower Baggot-street.

I heard a few days ago from this patient, and he is in excellent health, not the slightest tendency to any return of the disease.

Tumours in the neck, although of an innocent nature, yet from their size and attachments often embarrass the surgeon in forming his diagnosis, and in coming to a safe conclusion as to the best manner in which he ought to deal with them. When first they come under his observation, the important questions arise, What is the true character of the swelling? and can it be removed? or must it be permitted to run its course? In guiding him to safe practice he should, in the first place, endeavour to find out accurately the *age* of the tumour, and whether, during the period of its development, the general health of the patient had suffered or not. Secondly (if possible), discover the exact anatomical relations of the growth, how far it had displaced parts from their normal position, or invaded them by its pernicious influence. The following case of encysted tumour in the neck is, I conceive, invested with features of sufficient interest to warrant me in placing it on record:—

II.—*A Large Encysted Tumour of the Neck treated by Drainage and Injection—Long-continued Suppuration—Recovery.*

Bridget H., aged twenty-two years, unmarried, of dark complexion, was admitted into the Meath Hospital, May 14th, 1868.

History.—Five years previously she noticed that the left side of her neck looked fuller than the opposite, but she felt no pain nor the slightest inconvenience in breathing or swallowing. In about three months afterwards the centre of the general tumefaction

Fig. 1.

Fig. 2.

became elevated into a distinct tumour about the size of a grape; its situation corresponded with a spot one inch below the middle of the ramus of the inferior maxilla; was soft, compressible, and movable to the touch. She could not account for its presence, never having met with an injury, nor had any of her family ever been affected with glandular or other swellings in this situation. Her health was good in every respect, and her lower jaw and teeth were perfectly sound, The growth measured seven inches from above downwards, and five inches in the antero-posterior axis. Its shape was ovoid, and it occupied the whole of the left cervical region. It fluctuated freely beneath the fingers during examination; its surface was perfectly smooth, and it was painless; the skin was natural in colour; no distended veins wandered over its surface; it was not transparent, and when firmly grasped in the hand to test its mobility it felt tense, and gave the sensation as if it passed deeply into the neck; there was no pulsation, nor did excitement of any description enlarge its dimensions. Two days after her admission I plunged an exploring needle deeply into the swelling, and brought out in the groove of the instrument a small quantity of a fluid resembling pus.

Operation.—May 18th, 1868.—She was placed in a chair, with her head well supported by an assistant, who held it backwards, and slightly to the opposite side, and having had the most depending portion of the tumour whitened by the ether spray, I pushed an Erichsen's abscess knife into the cavity of the cyst, and having withdrawn it, I inserted the point of a probe with a piece of fine drainage tubing attached to its eye extremity. I then pushed the probe on until it elevated the sac and skin at its superior portion, showing distinctly where the instrument made them protrude. At this point I pinched up a fine fold of integument, and by transfixing it made a small opening in the skin. The point of the probe is thus easily made prominent without slipping, and can be laid bare, and forced through with the greatest facility; whereas, if the operator cuts on the skin made tense over the instrument, without this precaution he finds it more difficult to penetrate the parts, owing to their gliding over the blunt end of the probe. Having accomplished the transfixion of the tumour in this manner, I drew the probe, with the tube attached fairly, through its longer axis. Almost instantaneously the contained fluid began to exude from the perforations in the drainage tubing. It closely resembled pus, but was much thinner, and flowed with greater ease than

purulent matter. The ends of the tube were fastened together by a piece of silken ligature, and the tumour was covered with a thick layer of cotton wadding, held in its place by a bandage. The patient, who felt a little faint, was given some brandy, and placed in bed.

Six o'clock, p.m., the same evening, the tumour had reduced to half its former dimensions, but she had suffered great pain all the day, and was unable to swallow even fluids without considerable distress. She was ordered a full opiate at night, and to have the cotton replaced by a light bread and water poultice.

May 19th.—Notwithstanding the administration of the anodyne she had slept badly, owing to a feverish state, evidenced by thirst and great restlessness.

To note the daily changes in a case of long duration like this would weary the reader and frustrate my object in arresting the attention of the practical surgeon.

June 12th.—The thin discharge which escaped when the tumour was first opened had now nearly ceased, and in its place a dark-coloured highly fetid fluid flowed in small quantities through the tubing. I removed the drainage tube, and injected through the lower opening one ounce of a lotion, consisting of carbolic acid one drachm to four ounces of water, with one drachm of glycerine added. This I allowed to remain for four or five seconds, and then, by gentle compression with a softened sponge, I emptied the sac, and replaced the poultice. This injection was repeated every fourth day for a month, and although the discharge was not visibly decreased by its use, still the disagreeable odour had very much subsided. The former openings showed a great tendency to close; I therefore passed two threads of silk across the cavity, and tied their extremities together. A few days after the first injection I placed a large pad of charpie over the cyst, and made considerable pressure with a bandage, hoping to favour approximation of its walls. This gave intense pain, and caused a slight attack of erysipelas of the neck, which extended down on the upper part of her chest. The silk was now removed.

July 16th.—The tumour had decreased to one-third its original size, but she had evidently suffered much in health, having become thin, pallid, and complained of loss of rest from copious perspiration at night. Her appetite had become very bad. I now stopped all local measures, except injections of tepid water, and chiefly directed my attention to constitutional treatment to restore her weakened condition.

September 1st, 1868.—She is greatly improved in health; the tumour has subsided to the size of a large walnut, and the discharge consequently reduced to a small quantity; its character was healthy, and could be easily pressed from the cavity through the lower aperture; the upper had completely healed, having left but a trifling mark.

September 14th.—I again injected the carbolic lotion every second day for a fortnight with most beneficial results—namely, decrease of the discharge and diminution in the size of the growth.

The same treatment was persisted in until the end of January, 1869, when she left the hospital perfectly well, after a sojourn of more than eight months. The lithograph (Plate II., Fig. 1) gives a faithful representation of her original appearance when she first came under my notice, and Fig. 2 shows her improved condition on leaving for home.

This was, I consider, a case of very considerable practical interest. Could the case have been conducted in a more speedy manner with the same safety? I would say not. Any attempt to excise this cyst would have been attended with great danger, as it passed deeply among important parts of the neck, and independent of the difficulty of completely removing it, supposing the patient to have escaped the many consequences of such an operation, the mark from cicatrization must have produced a fearful amount of deformity which, particularly in a female, would have been highly objectionable.

The following was the result of a microscopical examination of the fluid contained in the tumour, which was made by my friend Dr. E. W. Collins:—

May 18th, 1868.—The fluid, when drawn off, was thin and purulent-looking. The fluid, in fact, to the eye, seemed thinnish pus. Nevertheless little pus was visible under the microscope. It chiefly consisted of pavement epithelium in a state of fatty degeneration, and plates of cholesterine.

ART. III.—*On Delirium Tremens.* By JAMES CUMING, M.A., M.D.; Professor of Medicine, Queen's College, Belfast; Examiner in Medicine in the Queen's University.

THE fundamental question in the pathology of delirium tremens consists in the determination of the precise relation existing between the disease and the alcohol to which it owes its origin. The tendency of medical opinion has been of late years to altogether reject the older theory which was long the accepted interpretation of the facts. From this two unfortunate results have followed. In the first place, facts totally incompatible with the current theory have been overlooked; and secondly, a very marked discrepancy has come to exist between the received pathological doctrine and the practice of the majority of physicians.

The great majority of recent writers regard delirium tremens as a specific form of poisoning. Its occurrence at a particular time is attributed to the fact that the poison has then accumulated in the system to the amount necessary for the production of the disease. According to this, which may be called the toxemic theory, delirium tremens is regarded as the cumulative effect of alcohol taken habitually for a considerable period of time, just as intoxication is the effect of a considerable quantity of alcohol taken within a short time. This view, with only unimportant modifications, is held, amongst others, by Drs. Peddie,[a] Laycock,[b] Gairdner,[c] Hughes Bennett,[d] Aitken,[e] Wilks,[f] and Anstie,[g] all of whom reject the notion that the withdrawal of alcohol has anything to do with the production of the disease; and although various authors have urged opinions adverse to this view, it must be admitted that their efforts have met with little success.

Having had many opportunities of studying the disease, both in hospital and in private practice, I have become convinced that this theory is inconsistent with observation, and that there exists, in a large number of cases, so distinct a connexion between the withdrawal of alcohol and the supervention of delirium that I have

[a] Edinburgh Medical Journal, June, 1854.
[b] Ibid., Oct., 1858, and Nov., 1862.
[c] Clinical Medicine.
[d] Principles and Practice of Medicine, fourth ed., p. 457.
[e] Science and Art of Medicine, Vol. i., fifth ed.
[f] Lectures on Nervous Diseases (Med. Times and Gazette, 1869).
[g] System of Medicine, Art. Alcoholism, Vol. ii., p. 76.

been irresistibly led to the conclusion that these two occurrences stand to each other in the relation of cause and effect. As it is by our notions of the pathology of the disease that our treatment must be ultimately regulated, I venture to offer the following remarks, which will be found to possess a direct bearing on this subject, and which may, I hope, lead to a reconsideration of it.

An attack of delirium tremens has been so often and graphically described that it is altogether unnecessary to enter upon any detailed account of it. The presence of tremors, the character of the pulse, the condition of the skin and tongue, the sleeplessness, and the peculiar delusions and hallucinations, enable the physician to recognize a well-marked case without the slightest difficulty. And although other varieties of mental and nervous disturbance may be brought about by alcohol, and although there is every reason to believe that an analogous condition may be produced independently of alcohol altogether, still the affection constitutes one of the best-marked clinical types which we possess. Now, it is important to bear in mind that these characteristic symptoms are by no means peculiar to the period during which the paroxysm of delirium is in force. There is scarcely a prolonged debauch at the end of which symptoms of a kindred nature are not observed; and it often becomes a question of no slight difficulty to be able to say whether or not a patient is about to pass into delirium tremens. The history of these minor types is of the highest interest in enabling us to make out the true nature of the more formidable variety. We find the hand tremulous, the tongue coated, a soft compressible pulse, perspiration, inappetence, short, broken, and unrefreshing sleep, dislike to remaining alone, and, not unfrequently, hallucinations when the eyes are closed. In cases still closer to the brink of delirium tremens we may even have delusions when the eyes are open, but the patient is able to master them. Now, the important point with regard to these cases is this, that these sufferings arise and are felt at the time when the individual has ceased to drink.[a] And undoubtedly the difficulty which is found in weaning drunkards from their deplorable vice depends in no small degree on the horror which they feel for this intermediate stage. It is a period evidently of intense

[a] Dr. Laycock (loc. cit., p. 411) seems to admit this to some extent, but he insists on the necessity of something additional, some con-causes, such as visceral affections; but the rapid recovery of the patient after a sleep shows that these are, in many instances, utterly insignificant.

nervous distress, and it requires great determination on the part of the sufferer to abstain from what he well knows will give complete present relief to his sufferings. He knows that his accustomed beverage will steady his tremulous hand, and enable his loathing stomach to retain food, and soothe his terrible nervous irritation, and give him, in a short time, temporary ease and comfort. A consideration of these facts alone ought, I think, to have saved physicians from drifting into the toxic theory with regard to the effects of alcohol upon the nervous system. We see that there are practically two great classes of effects produced by that powerful agent: the first, its direct or stimulant and narcotic effect; the second, its indirect effect when the system is becoming freed from its primary results. Just as in opium eating, we have the direct pleasurable effects brought about by the ingestion of the drug; and, secondarily, the distress and misery when the individual is either voluntarily or involuntarily deprived of it.

The same view is borne out fully by an attentive consideration of the phenomena of delirium tremens itself. We may take as the simplest example a case of the affection as it is met with in a man otherwise presumably healthy, who is in the habit of having fits of intemperance at intervals, with intervening periods of sobriety, or even of complete abstinence from intoxicating drinks. Members of the families of individuals afflicted with this deplorable tendency have told me that they have noticed a peculiar irritability and peevishness, and a certain amount of restlessness, to precede the indulgence in drink, and to give warning of its approach. The first days of the debauch are marked by the patient becoming drunk, often several times in the same day. After a few days, however, matters change in this respect. The individual whose case I am describing no longer becomes drunk, although drinking very freely, consuming probably over a quart of proof spirits in the twenty-four hours. He is certainly not sober, but he is equally certainly not drunk. He is oblivious of his obligations, reckless in his actions, maudlin, effusive, lachrymose, regardless of appearances, but he is talkative, plausible, and not devoid of cunning.* He displays considerable ingenuity in his endeavours to convince you that he does not really care for drink; that he is in some way the victim of circumstances, and that it is

* "On analysing the symptoms we are at no loss to perceive that it is the emotional and appetitive part of the mind which is in action, while the intellect, on the contrary, is directly enfeebled.—Anstie." Stimulants and Narcotics, p. 78.

his misfortune rather than his fault. It is, however, found to be impossible to prevent him from having drink, and he becomes very violent if any attempt is made to restrain him forcibly. He eats a good deal, especially at night, and he gets some sleep, generally during the day, taking an extra quantity of spirits when he wishes to go to sleep. When this state of affairs has lasted for a variable period, sometimes weeks, sometimes months, it is brought to a termination either voluntarily on the part of the individual himself, or by the exertions of his relatives. Sometimes it is a friend who undertakes to bring him round, partly by coercion, partly by management. Often is he put in charge of a sort of keeper who attends to cases of this kind, there being in Belfast, and, I presume, in most large towns in these kingdoms, men who devote themselves to this disagreeable but not unremunerative branch of industry. Or he may be sent to hospital; or, finally, his stomach may reject the drink, and in this way an enforced abstinence be brought about. Now, it happens very frequently that men of magnificent organization, when stopped in one or other of these ways, get well after a few days of horrible discomfort without passing through anything approaching to delirium. But if delirium is to ensue, a stage which now sets in is one which is of the highest interest with reference to the pathology of the disease. Suppose the supply of alcohol is cut off, what takes place is this, the man becomes thoroughly sober in twelve hours or so. He becomes perfectly rational, often deeply remorseful, and, in a word, in full and entire possession of his faculties. He is, to be sure, disinclined to, and, probably, incapable of continuous mental exertion, just as he is incapable of continuous physical exertion; he is miserably ill, but he is thoroughly clear in his intellect, and accurate in his perceptions. Now, I am anxious to insist on the importance of this stage, which furnishes, I think, the clue to the pathology of the disease, and the significance of which has been, if not overlooked, at all events not sufficiently insisted on by writers on the subject. A patient at this period, who had previous attacks of delirium tremens, has said to me, "I hope in God I am going to escape this time"—a hope which was not realized, as he passed into delirium tremens on the following day.

During this stage a patient will take nourishment, although usually unwillingly, will even sleep a little; and by this the physician is often led into the fallacious expectation that his patient is to escape an attack.

However, at a period varying from thirty-six to sixty hours, and, probably, in the majority of instances, under forty-eight hours, he begins to hear noises, or to see some imaginary object, and in a short time the attack becomes developed. Now I am aware that many excellent observers, among whom I may specially notice Dr. Peddie, of Edinburgh, who has contributed an excellent paper on this disease, state that delirium tremens usually comes on while the patient continues to drink. I am able to state, as the result of a not inconsiderable, although, of course, limited experience, that in no single instance in which I had an opportunity of personally observing the patient, did this occur. My experience may be exceptional, but it is positive on this point. Nay, more, I have witnessed the supervention of the attack at a period so uniform and regular, dating from the discontinuance of the alcohol, that I have been inevitably led to the conclusion, a conclusion quite opposed to what I was prepared to expect, that there exists a relation of cause and effect between these two occurrences. Given an individual who has once had delirium tremens; given, also, a certain period of indulgence, the length of which will, of course, vary greatly with the individual, and with the amount of alcohol, we may predict the supervention of delirium tremens within two days from the time when he ceases to drink with fully as much precision as we can foretel the period of maturation in a case of variola, or the crisis in a case of typhus fever.

I do not say that a continuance of his indulgence will indefinitely protect a patient, nor do I say that to encourage such a continuance is at all permissible, but I do say that it is quite within the power of the physician to determine the period when the attack shall supervene by depriving the patient of his drink. I am inclined to believe, as far as my own experience goes, which is, however, quite insufficient to determine such a point, that if a man continues to drink to great excess, and retains the drink on his stomach—an essential element in the problem—that it is not by delirium tremens that he would die: that convulsions or coma would probably supervene, or mania, or some of the other effects of alcoholism.

It is necessary to observe that the discontinuance of the alcohol may be either absolute or relative. I have several times witnessed the effects of complete abstinence. I have reason to believe that similar results follow from relative abstinence. For instance, I do not think that with respect to a man who had been drinking

twenty glasses of whiskey daily for a fortnight it would make any practical difference as regards the supervention of delirium tremens if he were allowed one or two glasses of whiskey in divided doses during the twenty-four hours.

Dr. Peddie quotes a passage from Dr. Blake's work, in which that author states that he could, at any time, have brought on an attack of delirium tremens in a habitual drinker simply by taking him into hospital for a few days, and keeping him on spoon diet. Dr. Peddie observes on this that Dr. Blake does not seem to have tried the experiment, and that it is an assumption from a theory supposed to be true.

I have tried the experiment. Several years ago, soon after Dr. Laycock's well-known and striking paper on the subject came into my hands, I had a patient removed to hospital at the urgent request of his friends. He was a man who, during the greater part of his life, had been in the habit of drinking for a few weeks at a time several times in the year, but who had never had delirium tremens. I did not put him on spoon diet; on the contrary, I allowed him an abundant supply of nourishment, but no alcohol. On admission he was somewhat under the influence of drink, but not much. It had long ceased to make him drunk; he was morose and rather stupid, but, as I have described, he became quite sober on the morning after admission, and complained bitterly of the restraint. On the evening of the second day he became delirious, and passed into one of the most severe attacks of delirium tremens I have ever seen. I have several times witnessed, although I have never since made myself responsible for a similar experiment. In eleven cases I have been able to ascertain that the paroxysm of delirium has followed the withdrawal of alcohol from the patients, in only three of which was the abstinence voluntary at first. It is extraordinary, however, how soon the desire for drink ceases in these cases.

I am anxious not to be understood as throwing any doubt on the published cases which point in a different direction from mine, but I may be allowed to say that the evidence in many of them is very incomplete, and that hospital physicians who rarely see cases of this kind before the delirium has set in are necessarily obliged to depend for the histories of their cases on those who accompany the patient, and that consequently these histories lose the most important guarantee of their accuracy—namely, the direct personal observation of those who record them. The friends of

a patient naturally attach greater importance to the weeks during which he has continued to drink than to the few days during which he has drunk little. I have myself seen no one case in which delirium tremens set in which the patient was alcoholized, and several medical friends of large experience in the disease have confirmed my observation on this point by their own.

I would by no means be understood to assert that delirium tremens may not come on while the man continues to drink. I merely state that I have not myself witnessed any such case. Several are recorded by observers of such eminence that I cannot hesitate to accept this as a form in which the disease may manifest itself. It is remarkable, however, that the experience of some observers should be so completely opposed to mine, Dr. Laycock, for example, having found that in twenty-one out of twenty-two cases the patient was alcoholized on admission into hospital.

In a number of other instances, no doubt, some debilitating or depressing influence, a shock, a convulsion, or, what occurs with great frequency, a smart epistaxis has been the starting point of the disease. In these instances, as in cases in which complications occur, as when injuries have been received by a man in a state of chronic alcoholism, or when some disease springs up, the problem of causation becomes more difficult. That in many instances the withdrawal, whether partial or complete, of the customary drink is, at least, a factor in producing the result, I believe to be certain. In others the supervention of delirium probably depends on the abnormal condition of the brain rendering it more liable to be injuriously impressed by those causes which have a tendency to produce delirium. It is often a most difficult and interesting question how far an alcoholic element enters into and accounts for some of the symptoms in certain cases of disease. But this portion of the subject is still enveloped in obscurity, and observations are not likely to be fruitful regarding it until some agreement is arrived at regarding the simple and uncomplicated cases.

What, however, has in my experience been invariable is, that a distinct interval of two days or a little longer has intervened between the reception of the injury and the development of symptoms of delirium.

How does such a succession of events as I have described tally with the cumulative or toxemic view of the pathology of delirium tremens? Is there any known substance capable of firstly producing a direct toxic effect; secondly, of remaining quiescent for

a period in the system; and, finally, of exploding with symptoms far more formidable than its first manifestations? It is reasonable to suppose that at the time delirium tremens begins, as I see it, there is less alcohol in the system than there had been at any given time for weeks, because elimination, and, perhaps, decomposition must have been going on during the interval of which I have spoken, and yet it is precisely at the time when we have the cause at its minimum that we have the effect at its maximum. To admit this we must, I think, attribute to alcohol something more nearly resembling the fabulous powers attributed to the secret poisons of the middle ages than any efficacy which the sober therapeutists of modern times will allow to the agents at their disposal.

It must have struck every physician how rapid and complete is the recovery from even the severest attacks. It is not merely that a man convalesces as from another illness, but he will eat a beefsteak heartily on the day after he has slept, and he will digest it with perfect ease and comfort. Yesterday he was in imminent danger of death; to-day he is almost free from suffering; to-morrow he will be able to go to his business. His disease has been *amarissimum naturæ pharmacum*, as Sydenham said of gout, but it has been a most effectual one. Are we to suppose that in those twelve hours of sleep the alcohol has been eliminated so effectually that no more trouble is to be experienced from it. I think this is hardly a possible explanation of the facts.

Are we, then, to accept the older explanation which refers delirium tremens to exhaustion from the withdrawal of a stimulus? I regard this as merely putting names in the place of facts. To me these terms convey no definite ideas such as my mind can grasp, and it is for this reason that in these observations I have studiously avoided all terms, such as stimulation, recoil, excitation, exhaustion, and the like. In the confusion which exists regarding their signification, their employment would only lead to additional possibilities of misconception.

The way in which I account to myself for the phenomena of delirium tremens is by reflecting on the perturbation in the animal economy produced by any sudden and profound change in the conditions under which its functions have been carried on. We know nothing of how morbific causes actually operate, but it is something to be able to group particular forms of action or suffering with others with which they bear pathological kinship.

We know that the functions have been going on while the organs were flooded with alcohol. Alcohol flowing with the blood, alcohol incorporated with the tissues. The patient slept and awoke, ate and digested; his bodily and mental functions were performed, *tant bien que mal*, in an alcoholic atmosphere. And this capacity of tolerating alcohol, it must be remembered, has been acquired by habit, in virtue of the wonderful power possessed by the human body of adapting itself to the most diverse conditions. You suddenly alter this in an important respect, and the organs must carry on their work under different conditions, and then, as always occurs in the organism under such circumstances, distress and disturbance ensue, even though the change be in the direction of the normal condition of matters. And as the change affects a condition mainly influencing the nervous system, the consequent disturbance is mainly shown in nervous functions.

This lead us, I think, to see that the analogies of delirium tremens are with the misery felt by the opium eater, or in a less degree by the tobacco smoker, when they discontinue the use of their narcotics, with the inconvenience also which is felt in the breaking off of many other habits; perhaps, too, with the agitation about the critical period in typhus fever which often reminds one a good deal of delirium tremens. All of these have this in common, that they are times of rapid transition.

So that in this way we, perhaps, might define the paroxysm of delirium as the expression of the disturbance caused by the transition from a state in which the functions of the nervous system have been carried on under one set of conditions to a state in which they are carried on under a different set of conditions.

It is evident that both the prophylaxis and the treatment of the disease must be based upon our conception of its pathology. Without entering upon any detailed consideration of the various methods which have been proposed, it is of importance to examine the grounds on which the administration of the two classical remedies—alcohol and opium—has been prohibited.

The instinct of most practical men has led them, in spite of theoretical considerations, to the adoption of a line of treatment of which a moderate amount of alcohol forms a part—an instance not uncommon in medicine of our practice being in advance of our theory. Dr. Laycock, for example, would allow a moderate amount of wine—a rule quite judicious, although hardly reconcilable with his theory of the disease.

Dr. Gairdner, whose remarks on the treatment of this disease are stamped with the great practical sagacity which characterizes his observations on medicine, has recognized the value of the occasional administration of alcohol as a *bribe* to induce the patient to take nourishment[a], and states that he is not inclined to relinquish this method in favour of any positive rule of total abstinence in the treatment of this disease. Now, if the malady be engendered by the presence in the system of alcohol itself, or of any of the products of its decomposition, or if it be even caused by the passage of alcohol in an unchanged state through the system, surely any addition of the poisonous element can only do mischief. It is contrary to all analogy, as well as repugnant to common sense, to admit the practice of administering a medicinal agent in small quantity for the purpose of remedying the injurious effects produced by the presence of the same agent in large quantity. Accordingly, Dr. Gairdner's practice, like Dr. Laycock's, must be regarded as in direct antagonism to the toxic theory of the disease.

I believe that it is the almost universal practice among physicians and surgeons to give alcohol in cases where delirium tremens arises in the progress of acute disease, pneumonia or rheumatism for instance, or after injuries in drunkards. And, finally, I may adduce the unanimous belief among a class of men whose opinions are surely entitled to some weight on this subject—namely, those who have themselves been the victims of alcoholic excesses—that it is in the highest degree dangerous to suddenly discontinue drinking.

My opinion with regard to the employment of alcohol in cases of this kind is that in many instances the occurrence of delirium tremens can be prevented by making the patient gradually diminish the amount of drink which he has been in the habit of consuming. But it must be done by slow degrees. It will not answer the purpose to give what would under other circumstances be a liberal allowance if it be much less than the quantity previously taken; and, as a general rule, I think that a daily diminution of from one-sixth to one-eighth of the total amount will be most frequently found to fulfil this indication.

It may be questioned whether in the case of a strong man it is not more advisable to allow delirium to supervene, on the ground that the disease runs a rapid course, and in such an individual is not likely to be attended with much danger. Moral considerations

[a] Clinical Medicine, p. 270.

also may intervene, but I have no doubt that it is for the interest of the patient to protect him, if possible, from the necessity of passing through this terrible ordeal.

When the attack has become developed, however, I have never seen it cut short by alcohol; and I think its administration should then be regulated by the amount of debility, by the presence of gastric irritability, and by the condition of the circulation. Certainly a large number of cases do well without it, but in others it seems to help to keep food on the stomach, and probably assists in promoting its digestion. I may add as a proof of the general confidence which the members of the medical profession have in its value that I have not myself met a single practitioner in the North of Ireland, who, as a rule, treats delirium tremens without alcohol.

Opium has been administered in this disease without injury in quantity far more than sufficient to cause death in a healthy person. So that it must be admitted that a marked tolerance of that drug exists in delirium tremens. I have recently had an opportunity of observing a case in which something like the converse of this occurred. In an opium eater, who was making great efforts to break off the habit, when the distress caused by the want of the drug had come on, after, for example, eighteen hours' abstinence from it, I found that brandy seemed to have very little effect. For obvious reasons I did not repeat the experiment often, but I convinced myself that neither alcohol nor carbonate of ammonia nor muriate of ammonia in full doses sufficed to make any distinct impression on the miserable discomfort which two grains of opium would relieve in a few minutes. It is a curious fact that a similar tolerance should be urged by some physicians, especially by Dr. Anstie,[a] to whose brilliant pen we are indebted for so much and so varied information on the subject of stimulants, as one among other evidences in favour of the benefits derived from the administration of alcohol in fevers. I am inclined to think that tolerance of large doses of a drug is not evidence either for or against its beneficial action; but that it comes under a general law that the nervous system, under conditions of intense action or suffering, becomes less susceptible of the influence of some agents which, under other circumstances, powerfully affect it. Thus, in acute mania, under strong mental excitement, in ecstasy, and the like, painful impressions are unfelt, and it is well known that in at least the two former opium has much less than its

[a] Practitioner for July, 1869; and Stimulants and Narcotics.

customary effect. The effect of severe pain in enabling large doses of opium to be borne without injury also comes under this category, and it is probably owing to the intense suffering in tetanus that enormous quantities of opium can be taken with impunity in that disease.

Dr. Anstie argues with much ingenuity[a] that the tolerance of poisonous quantities of opium or alcohol is to be explained by the fact that in the cases in which this tolerance is observed, from some cause or other it is found to be impossible to administer to the patient a sufficient supply of ordinary food. The history of cases of alcoholism does not bear out this view. It is precisely in instances in which very little food is taken that alcohol is found to have the most rapidly developed deleterious effects, and it is matter of ordinary observation that great excesses in alcohol can be borne with comparative impunity as long as a liberal supply of food is taken. And this fact has seemed to me to involve a difficulty which the advocates of the value of alcohol as a food are bound to explain. It is when little nourishment is taken that we should expect alcohol to be decomposed most rapidly, and its products most freely applied to the uses of the system; and it is when an ample supply of nutriment is taken that alcohol ought to exert to the full its pernicious or toxic powers on the nervous system.

Any one who has witnessed how completely even enormous doses of opium fail in producing sleep early in the disease will probably not be sanguine with regard to its beneficial effects. Nor will he expect much, at the same period, from the moderate doses which are recommended by some excellent physicians. But it is a different matter whether the occurrence of sleep may not be expedited by an opiate when the patient is nearing the termination of the disease. Dr. Sutton, whose tract on delirium tremens was mainly instrumental in inducing physicians to adopt the opiate treatment, did not recommend its administration early in the disease.[b] His cases, which certainly afford strong evidence

[a] Op. cit., p. 238.

[b] "A question may be here introduced in regard to the proper time of administering opium, and whether by a more speedy recourse to it success might not be more completely insured, and the most dangerous part of the disease prevented? To this I cannot reply so distinctly as I could wish. Considering the remedy as an unusual one for a disease of this kind, though highly efficacious, I have been cautious not to exhibit opium in the manner stated, except in such cases as appeared to me to be decidedly delirium tremens; and until the general concourse of symptoms are established this cannot be clear or definite."—"Tracts on Delirium Tremens," &c., by Thomas Sutton, M.D., 1813.

in favour of the line of treatment recommended by him, were not treated with opium till the disease had been for some time established. I believe that I have seen unquestionable benefit follow the administration of a dose of opium, or of Indian hemp, at the period which I have indicated.

If we bear in mind, however, what we have been taught by the celebrated observations of Ware, and by the results of the expectant treatment, that delirium tremens once established, enters into the category of morbid processes, possessing a tolerably definite course and duration, we will be slow to entertain any extravagant expectation of materially abbreviating its duration by therapeutic measures.

ART. IV.—*On the Mechanical Work done by the Human Heart.* By the Rev. SAMUEL HAUGHTON, F.R.S., Fellow of Trinity College, Dublin.

THE heart constitutes so peculiar and so important a muscle that it is worthy of a distinct consideration. It contracts constantly by means of a self-acting nerve supply, and works day and night, while life lasts, without ever experiencing a sense of fatigue, or the necessity for rest. Let us examine what is the mechanical equivalent of the work done by an average human heart, during the course of a single day and night.

Let Q denote the weight of the quantity of blood that is expelled from the left ventricle into the aorta at each stroke of the heart, and let h denote the height of a column of blood that would measure the fluid pressure inside the ventricle at the moment of contraction. It follows from the hydraulic laws of spouting fluids, that the blood Q, if thrown vertically upwards, would ascend to the height h, so that the work done in a single stroke of the left ventricle is represented by the product

$$Q \times h = \text{work done.}$$

We have now to estimate the value of the quantities Q and h.

The estimates given by various anatomists of the capacity of the left ventricle, vary from two to five ounces (water), the former estimate being that of the empty ventricle, which admits of considerable distension, especially during life. It is extremely difficult,

if not impossible, to say what is the amount of distension experienced by the left ventricle before it contracts upon its contents and expels them. From some experiments that I have made, it seems to me probable that the distension does not amount to more than fifty per cent., and that we shall not be far from the truth if we take Q to be three ounces of blood, neglecting the difference of specific gravity between blood and water.

Professor Donders[a] assumes the capacity of the left ventricle to be 188 grammes, which are equivalent to 6·631 oz. av., a result which seems to me more like the capacity of both ventricles, than that of one only.

In order to find the quantity h I shall employ the experiments of the Rev. Dr. Hales.[b] Having opened a large artery, such as the crural or carotid, in several animals, Dr. Hales inserted a brass or glass tube into the opening, and observed the height at which the blood stood before the animal had lost any large quantity of blood. The following are some of his results:—

No.	Artery	Height in Tube
1. Mare,	Left crural,	8·25 feet
2. Gelding,	,,	9·66 ,,
3. Mare,	Left carotid	9·50 ,,
4. Wether,	Carotid	6·46 ,,
5. Fallow doe,	Left crural	4·17 ,,
6. Dog,	Crural	6·66 ,,

An important fact was ascertained by Dr. Hales during the course of these experiments, viz., that when some of the blood was allowed to flow away, the force of the heart's contraction diminished, no doubt in consequence of the diminished resistance offered by the diminished circulation; the heart, as it were, instinctively measuring the work to be done, and expending upon it exactly the force required. The height of blood in the tube, which measured the force of the heart's contraction, became less and less as the blood was allowed to flow off, until it reached a minimum height, when the animal expired.

[a] Physiologie des Menschen (Leipzig, 1859), p. 109.
[b] Statical Essays (London, 1769), Vol. ii.

This minimum, carefully measured in the case of the horses, was found to be—

No.	Maximum Height before Loss of Blood	Minimum Height	Total Blood Lost
1. Mare,	8·25 feet	2·33 feet	924 cub. in.
2. Gelding,	9·66 „	2·83 „	895 „
3. Mare,	9·50 „	2·42 „	833 „
Mean,	**9·14** ft.	**2·53** ft.	**884** cub. in.

It appears from the foregoing table, that the minimum contraction of the horse's heart corresponding to the resistance offered by a circulation almost drained of its blood, amounts to the pressure of a column of blood 2·53 ft. in height. Dr. Hales has also recorded the important fact, that the crural or carotid artery of the horse, when first cut and allowed to spout freely, does not throw the blood much higher than 2 ft. It would appear from this observation that the cutting and free exposure of a cut artery to the air relieves the heart at once of the resistance offered by the capillary circulation, for otherwise it is difficult to understand why the blood should not spout upwards through nine feet, instead of somewhat over two feet.

Experiments on the pressure of the blood inside the human heart, similar to those of Dr. Hales on animals, have not yet been made; but it is estimated at 7·5 feet by Dr. Hales from considerations founded on the comparative bulks of the soft parts of man and of the horse supplied by the arterial circulation.

The following observation and calculation of the velocity of blood spouting from a large artery in man, lead, as I believe, to a more correct estimate, although indirect, of the pressure of blood inside the left ventricle.

On the 18th of March, 1863, a large fibro-cellular tumour was removed by Mr. M. H. Collis, in the operating theatre of the Meath Hospital, from the left groin of a middle-aged, large-sized man; in the course of the operation the external epigastric artery, which appeared enlarged to feed the tumour, was divided, and before it could be ligatured, strong jets of blood from it were thrown in various directions about the room. I noticed, as the

poor fellow moved about on the operating table, that the jets of blood fell short, or enjoyed a long range, according to the angle of elevation of the orifice of the bleeding artery, and that there was a certain maximum range on the floor of the theatre, which was not exceeded.

I saw immediately that I had before my eyes the solution of the problem that had puzzled me; as by measuring the co-ordinates of the maximum range, I could calculate the velocity with which the blood left the artery.

It is not necessary, here, to give the details of the calculation, which presents no difficulty. The height of the spouting artery above the floor was $3\frac{1}{2}$ ft., and the farthest squirt of blood was 8 ft. horizontally from the artery. From these data it follows that the velocity of the blood on issuing from the cut artery, is 12·905 ft. per second; corresponding to a pressure of 2·586 ft. of blood.

The result here found, shows that the heart of man, when experiencing the minimum of resistance, such as that offered by the circulation when a large artery is divided, or the vessels nearly empty of blood, contracts with a force measured by 2·58 ft. of a vertical column of blood. The heart of the horse, under similar circumstances, contracts with a force measured by a column 2·53 ft. in height.

The following considerations lead us to the conclusion that it is probable that the maximum hydrostatical force of man's heart is nearly the same as that of the horse. The experiments of Dr. Hales, fully corroborated by the recent observations of Poiseuille, show that the hydrostatic pressure of the blood is the same in all the arteries of the same animal that have a sensible diameter, from which fact it is plain that the resistance to the circulation of the blood takes place in the capillary arteries and veins. If, therefore, we knew the relative resistances offered in man and in the horse, to the capillary circulation, we could estimate the relative forces with which the blood is propelled in the circulation of these animals.

The experiments of Poiseuille on the discharge of liquids through capillary tubes, prove that the resistance offered by such tubes is directly proportional to the lengths of the tubes, and inversely proportional to the squares of their cross sections.

The quantity of liquid discharged by a capillary tube in a given

time, is inversely proportional to the resistance, and may be expressed in general by the following formula:—

$$Q = A \times \frac{h\, d^4}{l}.$$

In this expression, Q denotes the quantity of liquid discharged in a given time: A is a constant; h denotes the charge, or hydrostatic pressure of the fluid; and d and l are the diameter and length of capillary tube.

There is reason to believe that in animals similar in bulk, the arrangement and structure of the capillaries are such, that the ratio of the squares of their cross sections to the total lengths of the capillaries, is practically constant; as may be shown by a comparison of the sheep and the dog; the data concerning which animals I take from Dr. Hale's observations and experiments. The left ventricle of the heart of the sheep contains 1·85 cubic inches, and its pulse beats 65 times in a minute; the quantity of blood passing through the capillaries in a given time being proportional to the product of these two quantities. The hemastatical pressure of the blood in the large arteries was found by Dr. Hales to be 6·46 feet of blood.

The preceding equation gives us, for the quantity depending on the capillary resistance—

$$A \times \frac{d^4}{l} = \frac{Q}{h} = \frac{1{\cdot}85 \times 65}{6{\cdot}46} = 18{\cdot}6.$$

The average of the capacities of the left ventricles of the hearts of six dogs examined by Dr. Hales is 0·954 cub. in.; and the average statical pressure in the arteries of sixteen dogs experimented on by him, is 4·75 ft. of blood; and the average pulse of the dog is found to be 97 beats in a minute. Hence the equation will give us, for the quantity depending on the capillary resistance in the dog—

$$A \times \frac{d^4}{l} = \frac{Q}{h} = \frac{0{\cdot}954 \times 97}{4{\cdot}71} = 19{\cdot}6.$$

These results, depending solely on the resistance offered by the capillary vessels to the flow of blood, show by their near approach to equality, that in animals like the dog and sheep, we may safely assume the resistance offered by the structure of the capillaries to be nearly the same.

The sheep and dog differ from each other considerably in size of heart, rate of pulse, and hemastatical pressure of blood; yet, notwithstanding these differences, the *capillary coefficient* depending on them

all, comes out to be nearly the same in both animals. The capillary coefficient found by experiments made on the horse, can be shown to be double that of the dog and sheep, proving that the resistance to the circulation in the horse is only half that of the smaller animals.

The left ventricle of the horse contains 10 cub. inches, and the rate of pulse is 36 beats in a minute, while the average hemastatical pressure of the blood in the arteries is 9·14 ft. of blood; hence we obtain for the capillary coefficient in the horse—

$$A \times \frac{d^4}{l} = \frac{Q}{h} = \frac{10 \times 36}{9{\cdot}14} = 39{\cdot}3.$$

This quantity is double that found for the dog, and therefore the capillary resistance is one-half.

Dr. Hales did not make any observations on the hemastatical force of the arterial blood in the ox, but there is reason to believe that it is rather greater than in the horse, and nearer to 10 ft. than 9 ft. If we are at liberty to assume it to be 9·66 ft., which is the greatest height measured in the horse, we can calculate the capillary coefficient as follows:—In the ox, the capacity of the left ventricle was found by Dr. Hales to be 12·5 cub. inches, and the pulse beats 38 times in a minute. Hence the capillary coefficient in the ox is—

$$A \times \frac{d^4}{l} = \frac{Q}{h} = \frac{12{\cdot}5 \times 38}{9{\cdot}66} = 39{\cdot}8.$$

This coefficient is nearly the same as that of the horse, an agreement which serves to show that the capillary resistance depends more on the bulk of the animal than on any other condition.

If we suppose the coefficient of capillary resistance to be the same in man as in the horse, we can calculate the hemastatical pressure of the blood in the human arteries, as follows:—The human heart has a capacity in its left ventricle, when in action, of 3 ounces, or 5·2 cub. inches, and beats 75 times in a minute. Solving the equation for h, we have—

$$h = \frac{Q}{A \times \frac{d^4}{l}}.$$

Substituting for Q the product of the capacity of the ventricle and rate of pulse, and for the capillary resistance, its value found from the horse, we obtain—

$$h = \frac{5{\cdot}2 \times 75}{39{\cdot}3} = 9{\cdot}923 \text{ feet of blood.}$$

This is the value that I shall assume for the arterial pressure in

man. This estimate of the blood pressure approaches to that of Donders,[a] which is 10·527 ft. (3·21 metres).

We are now in a condition to calculate in foot tons, the daily work done by the left ventricle of the human heart, which lifts at each stroke, 3 ounces through a height of 9·923 ft. Its value is the following:—

$$\left.\begin{array}{l}\text{Daily work of}\\ \text{left ventricle,}\end{array}\right\} = \frac{3 \times 9{\cdot}923 \times 75 \times 60 \times 24}{16 \times 2240} = 89{\cdot}706 \text{ ft. tons.}$$

I find, on taking an average of the estimates of Bouillaud, Laennec, Cruveilhier, Sömmering, and Andral, that the right ventricle has a thickness, which is to that of the left in proportion of 5 to 13; and as the cavities are of equal dimensions, the thicknesses are proportional to their muscular powers; in order, therefore, to find the total work done by both the ventricles of the heart, we must add to the preceding result $\frac{5}{13}$ths of its amount, from which we obtain, finally,

Daily work of left ventricle, - - -	89·706	ft. tons.
Daily work of right ventricle, - -	34·502	„
Total daily work of both ventricles, -	**124·208**	ft. tons.

The total work done by the heart is somewhat greater than this estimate, for the auricles of the heart do some work, in propelling the blood into the ventricles; of which work no account has been taken in the preceding calculation.

The average weight of the human heart is found from the following table:—

Table of Average Weight of Human Heart.

	Oz.
1. Meckel, - - - - - - - -	10·0
2. Cruveilhier, - - - - - - -	7·5
3. Bouillaud, - - - - - - -	8·375
4. Lobstein, - - - - - - -	9·5
5. Boyd [æt. 30–40], - - - - -	10·41
6. Boyd [æt. 40–50], - - - - -	10·56
Mean = **9·39** oz.	

From the weight of the heart and the work done by it in one

[a] Physiologie, p. 109.

day, we can calculate the work done by the heart per ounce, as follows:—

$$\left.\begin{array}{l}\text{Work done by the human heart in}\\ \text{foot pounds per ounce per minute}\end{array}\right\} = \frac{124{\cdot}208 \times 2240}{9{\cdot}39 \times 24 \times 60} = 20{\cdot}576.$$

On comparing this result with the work done by other muscles, we find that the work done by the heart in a given time (20·576) exceeds considerably the work done by the muscles in a boat race (15·17). In a boat race the labour can be maintained for a few minutes only; while the heart labours at a greater rate throughout the whole of life.

There is another mode of exhibiting the wonderful energy of the heart, which is worth stating, more especially as it affords a means of comparing my own result with that of Prof. Helmholtz, the data of whose calculation I am not acquainted with. Let us suppose that the heart expends its entire force in lifting its own weight vertically, then the height through which it could lift itself in one hour is thus found, by reducing the daily work done in ft. tons to the hourly work in foot ounces, and dividing by the weight of the heart in ounces:—

$$\left.\begin{array}{l}\text{Height through which the}\\ \text{human heart could raise}\\ \text{its own weight in 1 hour}\end{array}\right\} = \frac{124{\cdot}208 \times 2240 \times 16}{24 \times 9.39} = 19754 \text{ ft.}$$

Professor Helmholtz[a] states as the result of his own calculations, that the heart could raise its own weight through 20250 ft. in one hour. It has frequently already stated that an active climber can ascend 9,000 feet in nine hours, which is only at the rate of 1,000 feet per hour, or one-twentieth part of the energy of the heart.

When the railway was constructed from Trieste to Vienna, a prize was offered for the locomotive alp-engine that could lift its own weight through the greatest height in one hour. The prize locomotive was the "Bavaria," which lifted herself through 2,700 feet in one hour; the greatest feat as yet accomplished on steep gradients. This result, remarkable as it is, is only one-eighth part of the energy of the human heart.

Professor Donders[b] gives an estimate of the force of the heart which exceeds considerably the result found by Prof. Helmholtz

[a] Medical Times and Gazette, 14th May, 1864, p. 529.
[b] Physiologie, p. 109.

and by myself. He assumes the contents of the left ventricle to be 0·188 kil., or 188 cubic centimetres, or 11·473 cubic inches, a result intermediate between the capacities of the hearts of the ox and horse, measured by Dr. Hales. Measured in ounces this capacity is 6·631 ounces, or more than twice the capacity assumed by me. The blood pressure is assumed by Donders at 3·21 meters. or 10·527 feet; and the force of the right ventricle to be one-third of that of the left ventricle. The other elements of his calculation are the same as my own. From these data, the daily labouring force of the heart comes out equal to—

87400 kilogrammeters = 281·03 ft. tons.

This result, as might be anticipated from the assumed size of the ventricle, is more than double that found by myself and Helmholtz, from independent data.

It may be useful to the reader to state here the results of observers subsequent to Dr. Hales, as to the pressure of the blood in the large arteries of several animals. Dr. Hales observed the maximum pressure in the arteries, as measured by a vertical column of blood, which was noticed to rise and fall. Subsequent observers have shown that in the anterior extremity the blood pressure depends on the respiration, being greatest during expiration and least during inspiration; and that this inequality is less sensible in the arteries distant from the heart. In order to compare the observations it is desirable to reduce Dr. Hales' column of blood to millimeters of mercury. The weight of a cubic inch of blood is 267·7 grs., while that of a cubic inch of water is 252·5 grs. The specific gravity of mercury is 13·596; and 9·14 feet (Dr. Hales' result for the horse) are equal to 2785 millimeters. Hence we find—

$$\left.\begin{array}{l}\text{Maximum mean pressure of blood in the horse, according to Hales}\end{array}\right\} = \frac{2785 \times 267{\cdot}7}{13{\cdot}596 \times 252{\cdot}5} = 217{\cdot}17 \text{ mm. of mercury.}$$

Poiseuille[a] directed his attention especially to the determination of the mean pressure of the blood, independent of expiration or inspiration; but has left in his memoir (p. 295) the data for calculating in the case of the horse the maximum pressure corresponding to that observed by Dr. Hales.

From the experiment there recorded on the carotid artery of the

[a] Journal de Physiologie par Magendie, Tom. viii., p. 272.

horse, I have calculated the maximum mean pressure of the blood during expiration, and find—

Maximum mean pressure in the carotid artery of the horse, according to Poiseuille } = 229·75 mm. of mercury.

The minimum mean pressure during inspiration in the same artery may be calculated from the data given at 63·61 mm. of mercury. The mean of all the maxima and minima being 146·68 mm. of mercury. The maximum pressure found by Poiseuille is a remarkable confirmation of the accuracy of the experiments made by Dr. Hales.

Poiseuille (p. 301) gives the following as the mean of all the pressures found in the horse and dog:—

No.	Horse	Dog
1	146·68 mm.	148·88 mm.
2	147·00 „	147·36 „
3	157·25 „	141·45 „
4	154·33 „	157·39 „
5	182·05 „	145·75 „
6	—	166·60 „
7	—	179·04 „
8	—	141·40 „
9	—	171·14 „
Mean,	**157·46 mm.**	**155·44 mm.**

Spengler[a] repeated the experiments of Poiseuille, and obtained, among others, the following results, from the arteries of the horse. The mean maximum pressures during respiration were—

1st horse,	...	...	229·31 mm. of mercury.
2nd „	...	...	200·67 „
3rd „	...	...	171·34 „
4th „	...	...	170·00 „
5th „	...	...	220·00 „
6th „	...	...	230·00 „
Mean maximum,		...	203·55 mm. of mercury.

This result agrees well with those of Hales and Poiseuille.

[a] Archiv. für Anatomie &c., von Müller (1844), p. 49.

The mean of all the pressures, including maxima and minima, were found by Spengler (pp. 58–9), to be—

No.	Horse	Dog	Goat
1	170 mm.	185 mm.	140 mm.
2	175 „	135 „	—
3	160 „	150 „	—
4	165 „	165 „	—
Mean,	**167·5 mm.**	**158·75 mm.**	**140 mm.**

The rate at which the blood circulates through the body may be estimated from the size of the left ventricle, as compared with the total quantity of blood in the body. The total quantity of blood in the body has been found by weighing, before and after death, criminals executed by decapitation. From such observations made by Lehmann, Weber, and Bischoff, the total quantity was found to range from 9 lbs. to 10 lbs. If we assume the latter quantity, and take the capacity of the left ventricle at three ounces, it is plain that a quantity of blood, equal to that contained in all the vessels, will have passed through the heart at the end of 53 strokes, which, if the pulse be assumed at 75 to the minute, will correspond to 42 seconds. This represents the time occupied by the entire circulation of the blood. The time occupied by absorption, circulation, and secretion combined, may be set down at less than four minutes, as appears from the following interesting experiment devised by Professor Macnamara, of the Royal College of Surgeons in Ireland, at which I had an opportunity of assisting. An old soldier, a patient of the Meath Hospital, suffering from hydrocele, was trained by us to urinate at the word of command, at intervals of one minute into a succession of test tubes. When his training had been completed by a few days' instruction, the sac of the hydrocele was emptied, and injected with two drachms of tincture of iodine. The patient then urinated at intervals of one minute into the test tubes previously prepared, commencing at one minute after the injection. The fourth test tube responded faintly to the starch test for iodine, and the fifth and subsequent tubes showed its presence very distinctly. From this experiment it is evident that the entire process, viz., absorption by the *tunica vaginalis;*

transmission by the lymphatics and capillary veins to the heart; re-transmission by the renal arteries to the kidneys; secretion of urine and its transmission to the bladder by the ureters, occupied less than four minutes, or about five times the entire time required for the circulation of the blood.

ART. V.—*On Excision of the Wrist-Joint.* By JAMES F. WEST, F.R.C.S. (Exam.), Professor of Anatomy in Queen's College; and Senior Surgeon to the Queen's Hospital, Birmingham.

THE operation of excision of the wrist-joint has been so seldom performed, as compared with excision of the larger joints, as the elbow and knee, and its results have been so little recorded, that I consider it only right to bring under the notice of the profession, the particulars of some cases which have come under my observation. In each instance the operation has been performed some years, and therefore a fair opportunity is afforded for judging of the ultimate result of the operation in suitable cases.

I shall describe, as briefly as possible, the condition and progress of the patients prior to the operation. I shall give in detail the particulars of the operation, and the course of treatment adopted subsequent to its performance. I shall then endeavour to draw some inferences from these, and other resections of the wrist-joint, which have been done during the last few years, as to the most suitable manner of performing the operation; and, lastly, consider the circumstances which would determine the advisability of excision of the wrist, in preference to amputation of the forearm in cases of extensive injury or disease of the wrist-joint.

Mary Horton, married, aged forty-three, became an out-patient of the Queen's Hospital, Birmingham, in the beginning of April, 1865, with great enlargement of the right wrist-joint, which had existed more or less for three years, and which was owing, according to her account, to a needle having run into the ring finger of the right hand while she was engaged in washing some clothes. The needle was removed entire, but shortly after a swelling formed in the palm of the hand, which, getting larger, extended to the front of the wrist in one direction, and up the fingers in the other, and caused the latter to become painful and stiff. The pain, swelling, and stiffness had rendered the hand and fingers perfectly useless for many months; no abscess had formed; the

swelling was brawny and dense—was most marked over the wrist-joint, particularly on the dorsal aspect, but afforded only an indistinct sense of fluctuation. She stated that she had never had either acute or chronic rheumatism; that her general health was good, although her family were delicate—her mother and only sister having died of consumption; that she had lived very poorly of late, her husband having been out of work; that she was the mother of four children, whom she endeavoured to support by working as a washerwoman.

She was pale and weak; suffered greatly from pain, which prevented her resting at night, and gave her a very enfeebled and emaciated appearance. She was ordered citrate of iron and quinine, generous diet, and a sedative draught to be taken each night.

The wrist-joint was enveloped in Scott's dressing, and supported on a splint in a prone position.

It is needless to give a detailed account of her progress during the next six weeks; suffice it to say that the pain and swelling increased, hectic symptoms showed themselves, and an abscess, pointing at the back of the wrist-joint, was imminent, when I decided to resort to excision of the wrist-joint; the patient, after having been informed of the critical position in which she was placed, preferring to subject herself to the risk of that operation rather than to submit to the loss of the right hand and part of the forearm.

Accordingly, on the 24th of May, 1865, the patient was brought into the operating theatre, and placed under chloroform. A tourniquet having been lightly placed over the brachial artery, so as to command that vessel in case of hemorrhage, I commenced my first incision on the dorsal surface of the hand, about two inches below the styloid process of the radius, and just to the ulnar side of the extensor secundi internodii pollicis tendon, and keeping this as my guide, continued it upwards to about three inches above the wrist-joint.

I then made a second incision of an equal extent along the ulnar side of the back of the hand, and the lower part of the forearm. Both incisions were carried carefully down to the wrist-joint, which was then opened, the hand being forcibly bent downwards while this was done, and while the external lateral ligament of the wrist-joint was being divided. The extensor tendons of the thumb were then drawn outwards towards the radial side, and, protected by a spatula, while the handle of the scalpel was passed beneath the tendons of the extensor communis digitorum and extensor minimi

digiti, so as to liberate them as much as possible from the fibrinous deposit in which they were embedded, and to enable them to be moved to either side of the forearm, as was necessary in the subsequent part of the operation. The internal lateral ligament and the extensor carpi ulnaris tendon were then divided, and the lower end of the ulna having been pushed through the ulnar wound, was cut off with bone forceps just above the styloid process. I had more difficulty in getting the radius exposed; careful dissection was necessary on the palmar surface, the edge of the knife being kept as close as possible to the radius until the bone could be forced through the opening on the radial side of the forearm, when the common extensor tendon having been drawn to the ulnar side, and the extensor carpi radialis longior and brevior tendons having been divided, about one inch of the radius was removed by Butcher's saw, which was for that purpose introduced on the anterior surface, and made to cut backwards and upwards.

Both radius and ulna were found to be carious, as also the first row of carpal bones, which were now removed one by one.

This was the most tedious part of the operation, each requiring to be seized with small, but strong, bone forceps, and carefully dissected out, the knife being kept as close round the surface of the bone as possible, so as to prevent its point from getting deeply into the structures on the palmar surface of the hand. All these bones were more or less carious, and some difficulty was experienced in severing them from their connexions with each other, and with the surrounding parts, in consequence of the great amount of fibrinous infiltration which surrounded them. The second row not appearing at that time to be involved in the disease, were suffered to remain. Not much hemorrhage occurred during the operation, and only one vessel required the application of a ligature; the edges of the wounds on either side of the hand were approximated by three intercepted silver sutures, and the hand having been placed on a well-padded straight splint, in a position of pronation, was supported by a bandage, and the patient then removed to bed.

No bad results followed the operation; the wounds partially healed by the first intention, and the patient's condition improved sensibly. The splint was changed for a shorter one, which only extended as far as the commencement of the fingers, a week after the operation, and she was ordered to have the fingers moved a little every day. A daily record of the patient's progress showed that, although her general condition steadily improved, still a

considerable purulent discharge kept up from the arm, and it became evident that mischief was going on in the carpal bones, which had been suffered to remain. In September, my colleague, Mr. Wilders, who kindly took charge of my patient during my absence from home, found it necessary to remove some loose fragments of carious bone from the carpus, but the parts were so altered by disease, that it was almost impossible to say what were the exact parts removed, but from their situation they were judged to be parts of the os magnum and unciform.

In October a small piece of bone came away with the discharge, and at the end of that month she was able to leave the hospital. During the following month two more small carious fragments were also discharged. Since then no bone has either come away or required to be removed, but the parts have slowly, but gradually, consolidated. A few small abscesses formed during the early part of 1866, but these did not appear to communicate with any fresh caries of the bones. Her recovery would probably have been more rapid, had she been perfectly supplied with nutritious food, and been able to get to a convalescent institution, or to the seaside, but during the greater part of the time which has elapsed since the operation, her husband has only been partially employed, and his wages never exceeding a pound a week, she has not been able to have such a diet as was really necessary for a woman in her condition. To add to her difficulties she became *enciente* towards the end of 1866, and she was delivered of a healthy female child on July 12, 1867, which she nursed for several months.

The arm now presents the appearance shown in Plate IV., Figs. 1 & 2; it is, of course, considerably shortened, but there has been a great amount of osseous and ligamentous deposit between the metacarpal bones and the lower ends of the radius and ulna, and the parts feel firm, and bear pressure everywhere without causing her pain; she is able to pick up a sheet of paper or a pen from the table, and can without difficulty approximate the thumb and forefinger. She tells me that she is able without assistance to dress her baby, and can bear its weight on her right forearm, at the site of the joint, without inconvenience; sensation is perfect in all her fingers, and slight power of flexion exists in the distal phalanges, but any attempt to bend the proximal ones, or to close the hand, causing her pain, she refrains from doing so. I have several times bent the fingers for her, and have advised her to keep bending them herself, but I fear she does not do so as much as she

Fig. 1.

Fig. 2.

ought; the fingers are now free from swelling, and she is gradually accustoming herself to use them more and more.

I append a comparative statement of the measurement of the sound arm and of that from which the wrist has been removed, in various situations, taken recently; from which it will be seen that the amount of alteration in size is not so great as to cause marked deformity, and certainly less than might be expected:—

	Left Arm.	Right Arm.
Measurement from tip of olecranon to end of little finger - - - - -	14½ inches	14 inches.
,, ,, ,, middle finger	16 ,,	14 ,,
,, ,, ,, thumb -	14¼ ,,	13 ,,
,, around the wrist - - -	6 ,,	6½ ,,

William Hancox, aged twenty-three, porter, Birmingham, admitted into the Queen's Hospital, April 18, 1867.

On admission stated that sixteen months ago he noticed, on getting up one morning, a swelling on the back of his right wrist, accompanied by a good deal of pain on moving the hand; he was ordered to keep his bed, and apply poultices to the wrist, by his medical attendant. In about three days he complained of great pain in the joints of the extremities, accompanied by fever; he was confined to his room for about six weeks with rheumatic fever, at the end of which time he was able to resume his employment. During the time he was confined to his room the swelling of the wrist had been opened by a surgeon, and a quantity of pus let out. In the course of a short time six or seven small abscesses formed in the neighbourhood of the joint, and discharged small quantities of pus; the openings thus formed did not heal, but continued discharging more or less until his admission into the hospital. The joint was rendered useless, and the fingers could not be moved. He stated that he had always enjoyed good health previous to this rheumatic attack, and that his relations were all healthy. For the last four months he had been unable to use the joint sufficiently to enable him to attend to his usual occupation; various remedies have been applied, but without the smallest benefit. On examination, the right wrist-joint was found to be greatly swollen, with numerous sinuses situated over the posterior surface of the joint, especially on the radial side, all discharging freely, and all leading to diseased bone.

The patient, from being a stout-built healthy man, was reduced

to a mere skeleton; the exhausting pain and profuse discharge having evidently greatly reduced his constitutional powers.

April 24th.—After the administration of chloroform I ascertained, by careful probing, that the chief part of the disease was in the radius, the sinuses over the ulna, although leading down to that bone, not finding it exposed, while the carpal bones also appeared free from caries.

I therefore determined to make less extensive exposure of the parts than in the previous case, and simply contented myself with a single longitudinal incision in the median line of the forearm, from a point three inches above the wrist to one about one inch below, and joining this about opposite the joint with a superficial transverse incision, an inch in length, extending towards the styloid process of the radius.

I carefully deepened the longitudinal incision until I had exposed the extensor tendons, and then, having separated them at their middle with the handle of the scalpel, I made my way down to the wrist-joint, which I opened freely on its dorsal aspect. The hand was then forcibly bent forwards and backwards, so as to free the parts, and make them more mobile, and then the lower ends of the radius and ulna were minutely examined, the tendons being held aside by retractors, and every precaution being used to prevent the division of any of them. A cavity large enough to hold a marble was found in the lower end of the radius, and the bone for an inch above it was found to be soft and carious. The external border of the radius was therefore cleared about two inches above the lower end of that bone, the superficial transverse wound before mentioned giving sufficient room for the movement of the knife, the handle of which was more used than the blade in the separation of the pronetor quadratus, and the soft parts lying on the anterior surface of the forearm. A curved needle, armed with a ligature, to which the chain saw was attached, was then passed around the radius at this point, and brought out through the interosseous membrane, and the handle having been applied to the saw, the bone was rapidly cut through. The lower fragment was then seized with lion-bone forceps, and dragged downwards, the point of the knife dividing any attachment to subjacent parts, and finally drawn from its place, after division of the tendon of the supinator longus, and the external lateral ligament. The lower end of the radius having so long been the seat of caries, and pus having existed in the joint, it seemed not unlikely that the lower surface of the ulna might be involved

in the disease, even though its shaft was evidently free from it. To make quite certain of the matter, I thought it advisable to force the hand downwards, and towards the ulnar side, but not being able by this means to obtain a sufficient view of the parts, I divided the internal lateral ligament, and was then able to bring the lower end of the ulna through the wound on the dorsal surface of the wrist-joint, I found that it was free from caries. I therefore returned it to its place, and having then examined the first row of carpal bones, and seen that they were all free from disease, I left them undisturbed.

No vessels required ligature. I closed the wounds by four silver sutures; supported the forearm and hand on a splint in a prone position, and with a few turns of bandage, above and below the joint, steadied the limb; cold spirit lotions were then applied.

April 25th.—Slept the greater part of the night; he felt very comfortable; pulse 100; ordered to have mutton diet, with a pint of beef-tea; an opiate if necessary.

April 27th.—Suffers little or no pain in the wound, and the limb preserves a nice temperature; pulse 90; tongue very red, but not dry; bowels open; ordered four ounces of sherry; limb re-bandaged, and water-dressing continued as before.

April 30th.—Sutures removed; greater part of both incisions united. Free discharge of healthy pus. Takes animal food well, and is in excellent spirits. Splint applied as before, but with rather more padding on radial side.

May 4th.—Profuse suppuration. Eats, drinks, and sleeps well. Wrist to be dressed with lotion of permanganate of potash.

May 8th.—Continues to improve; discharge diminished; has been moving about the ward for the last three days; the limb resting on the splint, and being supported in a sling, but the ends of the fingers left free. To take two grains of quinine three times a day.

May 14th.—There is still great discharge, and the old sinuses remain open; health is not good; goes out on the green every day, and lives well.

May 28th.—Parts still puffy and swollen; discharge abundant; no pain in the part, except when it is dressed, or when the fingers are moved; ordered to go to the Sparkhill Sanatorium. There he remained till August, steadily improving, the arm diminishing in size, the sinuses many of them closing, and his health becoming vastly more vigorous. About the middle of that month he caught

cold from exposure to wet; rigors came on, and were succeeded by great febrile excitement; an abscess formed up the forearm on the radial side, and many of the old sinuses began to discharge afresh. On August 24th he was re-admitted to the hospital; several sinuses were laid open, and some of them which communicated with each other were connected by Chassaignac's drainage tubes. These were introduced by our excellent house surgeon, Dr. Jolly, who throughout has taken great interest in the case, and to whose careful dressing the favourable result is in no small degree to be attributed. The drainage tubes were ordered to be syringed out with the permanganate of potash lotion every night and morning. He was also ordered a chop, beef-tea, milk, and four ounces of sherry daily, and to take two grains of quinine, with 3i. of tincture of hop, and five drops of dilute sulphuric acid three times a day. Under this treatment he soon improved; the discharge diminished, so that at the end of a fortnight the drainage tubes were removed; the fistulous openings began to close, and the only inconvenient symptom was a great amount of soft swelling over the lower end of the radius. To obviate this he was ordered to have the arm more firmly bandaged, and to have pressure kept up by means of some thin sheet-lead applied round the lower part of the forearm.

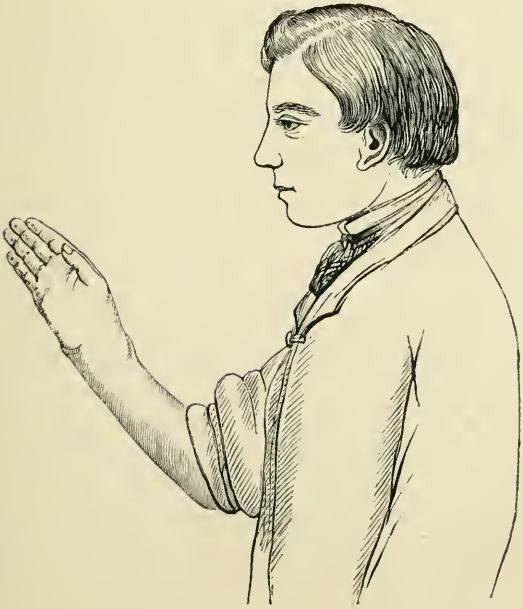
Towards the end of October, the short splint, leaving the fingers quite free, was resumed, and free movement of the fingers was made by Dr. Jolly every day; the sheet-lead was still used, as the swelling was gradually decreasing under its use; his health was again good, and he was allowed to go out of the hospital for a walk every day.

During November the sinuses gradually healed; the puffy swelling went down, and in its place could be felt firm fibrinous deposit at the site of the excised bone. The power of flexion and extension of the fingers returned to a great extent, so that he could pick up a book, or hold a pen.

At the present time all the fistulæ are healed. The parts can be handled without causing him pain, and they feel firm and compact; the styloid process of the ulna projects in rather an unsightly manner, but, with that exception, the appearance of the joint is admirable (Plate V., Figs. 1 & 2), and his power of using it, and also of flexing or extending the fingers, is so perfect that he is, and has been, able, ever since he left the hospital, to carry on his employment as porter in a large drapery establishment without any difficulty. I give a fac-simile of his handwriting, and he can lift a heavy weight.

Fig. 1.

Fig. 2.



	Left Arm.	Right Arm.
Measurement from tip of olecranon to end of little finger	15 inches	$12\frac{1}{2}$ inches
,, ,, ,, middle finger	17 ,,	$16\frac{3}{4}$,,
,, ,, ,, thumb	$15\frac{1}{2}$,,	$13\frac{1}{2}$,,
,, around the wrist	$7\frac{3}{8}$,,	$8\frac{3}{8}$,,

J. F. West, Esqr., F. R. C. S.
Queen's Hospital,
Bath Row,
Birmingham.

Excision of the wrist appears to be an unpopular operation even with those who are the most strenuous advocates for the excision of other joints; this is probably due in no slight degree to the rules for its performance being less clearly laid down than those which experience has assigned as the most suitable for other excisions, but may also be attributed to its having been less frequently performed, in consequence of a rather ill-founded prejudice against it.

I believe that a more extended trial of the operation will lead to its being received into the general list of operations, which are not only justifiable, but also necessary in well selected cases.

Mr. Holmes, in his excellent essay on "Excisions of Joints," in Vol. iii. of the *System of Surgery*, states that "no operation appears advisable when the whole carpus is involved in chronic disease," alleging, as a reason, that the patient may recover if the strumous cachexia is not too far advanced, while if it is, he will not recover from the operation more than from the disease.

If the same line of argument were used in cases of caries of the bones entering into the formation of the elbow or knee, it would be necessary to decline to operate in almost every case where resection of those joints is now performed. Either an expectant treatment or amputation would have to be recommended; yet resections of the

elbow and knee are daily performed, for the purpose of getting rid of the carious bone, and so relieving the strumous cachexia by which the utility of the joints have either been destroyed or impaired, and so of giving the patients a chance of a useful arm or leg. That the whole of the carpal bones may be removed, even in strumous subjects, and yet the patients live for a considerable time is evident, from the record of these, and other cases published by Professor Lister and Sir Wm. Fergusson; while I think that it must be admitted that, even supposing the hand preserved has but little mobility, as Mr. Butcher concisely puts it, "a stiffened hand and wrist is better than no hand at all."

I do not believe that as an operation it is so barren of success as to deserve to be stigmatized as it is by M. Malgaigne, who, after reviewing the various modes of performing the operation, and the cases in which it has been employed, expresses this opinion, "C'est donc um operation deplorable, et qu'il est temps de rayer absolument de la pratique."

I may cite in opposition the case of the soldier from whom, in 1762, Beyer removed the whole of the bones entering into the formation of the wrist-joint, together with the metacarpus, for a gunshot wound, in whom recovery followed, and the patient is said to have preserved the form of his hand.

Also the case in which M. Dietz, in 1839, took away the articular ends of the radius and ulna, together with the bones of the carpus, from a man, forty years of age, who lived four years after the operation, although he then had to submit to amputation, in consequence of caries occurring in the upper ends of the metacarpal bones.

I do not think that, although the case recorded by Mr. Butcher was followed by death in eight days, the result was due to the particular nature of the operation performed, as it is stated (*Operative and Conservative Surgery*, page 211) that "there was no local manifestation of mischief; on the contrary, repair had rapidly advanced;" but was due to such circumstances as are common to all operations, and might have occurred to the patient had amputation of the forearm been performed instead of excision of the wrist-joint.

Where only the lower end of the radius or ulna is affected singly, cases ending favourably are even more numerous than where both bones of the forearm are implicated; many cases might be cited, but two examples will suffice:—

In 1841, M. Ricord removed the lower third of the radius of a

young man, twenty-three years of age, who was the subject of caries—the operation performed being almost identical with that adopted in my second case, and with an equally good result: the patient recovering in the course of six months, with a perfectly useful hand.

In 1862, I removed, by a single longitudinal incision, about two inches of the lower end of the ulna of a woman, aged thirty-six, who had for thirteen years been troubled with abscesses about the forearm, for which she had been recommended by Mr. Wood to submit to amputation of the forearm at the General Hospital, Birmingham. Necrosis had resulted from a severe fall against a chair, which, to use her own words, "had bruised the bone." The line of incision healed rapidly, so that the patient was able to leave the hospital in a few weeks, but during the succeeding twelve months three small pieces of carious bone came away, and then the parts became firmly cicatrized, and no further trouble has since arisen about the wrist. She has now a useful hand, both pronation and supination being well performed. The lower end of the ulna is nicely rounded off; the forearm is shortened about an inch on the ulnar side, and the little and ring fingers are slightly flexed up on the palm, but can be easily extended, and both of them have preserved both their sensibility and mobility.

These cases may, perhaps, not be looked upon as excisions of the wrist-joint. They are clearly not complete excisions, but in cases of excision of the elbow the amount of bone removed is very variable; the head of the radius often, when unaffected by disease, is left intact, and yet these cases are none the less recorded as instances of excision of the elbow-joint. Again, in the cases of so-called excision of the ankle, the parts removed have rarely included all the bones entering into the formation of that joint. In excisions of the knee-joint, the patella, if unaffected by caries, is not generally removed by the surgeon. It is sufficient for the purpose of the surgeon to remove whatever portions of bone are affected by caries at the time of the operation, and to leave untouched those that are not obviously affected by that disease. In the early cases of excision of the wrist it was the practice to make an oval or semicircular incision down to the wrist-joint, dividing in this way all the tendons. In the operations performed by M. Velpeau and Mr. Stanley, all the tendons, and even in Mr. Butcher's case the extensor tendons of the four fingers were cut; but in the more recent cases by Sir W. Fergusson and Professor Lister, all the tendons

of the thumb have been left, and also those of the common extensor of the fingers. I believe increased experience will prove that however much they have been matted together by inflammatory exudation, it is better to avoid cutting these tendons, and that the result obtained thereby will be a far more useful hand than if they are divided in the course of the operation.

In deciding as to the cases most likely to be successful after excision, much will, of course, depend on the age and constitutional powers of the patients. In children and young people caries commencing in the carpus is so apt to rapidly progress from one bone to another, and also to involve the meta-carpal bones, that the operation, if performed, should be done early. In all cases a good result is more likely to follow if the caries is only limited in extent, and consequently a smaller amount of osseous structure has to be removed. There can be no doubt that when caries commences in the articular end of either radius or ulna, those parts should be removed early (as in the case of William Hancox) before the disease has time to extend to the carpus. There are numerous cases on record showing the good results of removal of the lower ends of both radius and ulna, either in severe compound fractures of those bones involving the wrist-joint, or for extensive caries. Chassaignac ("Operations Chirurgicales," tome premier, page 630) gives the particulars of many successful cases occurring in the practice of Clemot, Guepartte, Hublier, the elder Moreau, and Adelmann, where it is evident that the articular ends of the radius and ulna, as well as the carpal bones, had been the seat of caries.

I think the best and easiest way of performing the operation is by two longitudinal incisions extending along either side of the dorsum of the wrist, from about two inches above to two inches below the joint—the radial incisions keeping to the ulnar side of the tendon of the extensor secundi internodii pollicis, and the ulnar being rather on the anterior surface of the ulna, but close to that bone, and extending as far as the base of the metacarpal bone of the little finger. These incisions, if sufficiently long, will enable the operator, without much difficulty, to get away all the parts affected by disease, and no transverse incision will be needed. None of the extensor tendons of the thumb should be injured, nor should the common extensor of the fingers on any account be divided. They may, after being freed from the subjacent parts by the handle of the scalpel, be drawn to either side, according as either the radius or ulna is being operated on. By using the

chain-saw the parts are much less disturbed than when Butcher's saw is employed; the radius or ulna must be firmly steadied by an assistant holding the forearm, while the fragment to be removed is secured by the lion forceps in the hand of another assistant. Too much importance cannot be placed on early movement of the fingers; probably a week or nine days after the operation it will be advisable to shorten the splint, so that it does not extend beyond the base of the fingers, and then motion should be commenced by the patient himself, or by the nurse in attendance. Free openings should be left for discharge of matter during the progress of the case, and drainage tubes may be introduced with advantage when sinuses remain in the neighbourhood of the joint.

Probably six to twelve months will elapse before there is much consolidation of the parts at the site of the excision, and before the hand can be of very great use; but even if three or four times that period is required before much benefit is evident, the patient will be far better off than if he had submitted to such a formidable mutilation as the loss of a hand.

Good nourishing diet, and removal to the pure air of the country, are, no doubt, as after all operations, of the greatest importance. Had my patient, Mary Horton, been able to avail herself of these circumstances, I cannot doubt that her progress would have been more rapid. In Hancox's case removal to a Sanatorium some miles from Birmingham was followed by the marked improvement of both the local and general condition.

In conclusion, I trust that my surgical brethren may be induced to give this operation a more extensive trial than it has yet enjoyed, and I am confident that in the hands of many of them even better results may be obtained than in those cases which I have narrated.

ART. VI.—*On Excision of the Ankle-Joint, being an Account of the Second Case in which that Operation was Performed in Britain.* By HENRY MURNEY, M.D., M.R C.S., Eng., &c., &c.; Surgeon Belfast General Hospital.

W. C., aged fifteen, was admitted to the Belfast General Hospital 10th May, 1856, labouring under disease of the left ankle; states that on the 26th December, 1854, he experienced most acute pain in the joint, accompanied by swelling. The pain was much

aggravated by pressure or motion of the joint; believes he must have hurt or strained it, but cannot recall the exact occasion; from this time he never completely recovered the use of the articulation. He was treated at first at home, but matters becoming worse he was admitted into hospital, where he had rest, and afterwards counter-irritation. He was discharged on the 24th June a good deal improved. A fresh accession of suffering caused him to seek re-admission on the 14th February, 1857. He is a delicate, strumous-looking subject, smart and intelligent. As yet the appetite is good; he sleeps well, and he has not lost flesh. There is considerable swelling around the ankle, especially at the sides of the articulation, but there is no swelling or tenderness to indicate that the astragalo calcaneal joint is involved. The slightest motion causes pain for a considerable time. The joint was put up in splints to give complete rest, afterwards starch bandage was applied for the same object; mild counter-irritation, and then more severe irritants were resorted to, with temporary benefit. But after two months' trial his general health was beginning to suffer, and his appetite to fail. It was quite apparent operative interference was now necessary, and a consultation was held. All agreed on the propriety of removing the diseased parts, but some difference of opinion existed as to whether the case was an appropriate one for excision, or whether a better prospect would be afforded by amputation at the lower part of the leg. The principal difference being how far the tibia was involved, it was finally arranged that the articulation should be opened, and if, as was hoped, the disease was of limited extent, excision would be performed; but if it occupied much of the leg bone amputation must be resorted to. On the 21st April, 1857, the operation was performed as follows:—

An incision was commenced at the posterior margin of the tibia, about an inch and a half from its lower extremity, and carried downwards below the malleolus, then curved forwards and outwards across the instep to the outer malleolus, behind which it was carried straight up along the fibula as high as the incision at the inside of the limb. The vertical portions of this incision were cut down to the tibia and fibula at once; that across the instep was carried through the extensor tendons and the dorsalis pedis vessels. A ligature was applied to the bleeding artery, and the lateral ligaments were cut. Now that the joint was open it was determined to excise. The saw was applied to the neck of the astragalus, and the bone cut obliquely downwards and backwards, emerging at the posterior part, and

Dr MURNEY'S CASE OF EXCISION OF THE ANKLE.

slicing off the trochlear surface with the upper portion of the facets for articulation with the malleoli. The remainder of these were taken off with the bone forceps, and lastly, the lower articular surface of the tibia with a very thin layer of bone, and the two malleoli was removed with the saw.

The synovial membrane had undergone the *brown fibro-gelatinous* degeneration, and the cartilage was completely removed from several points of tibia, fibula, and astragalus, while that which remained was much thinned and readily separated from the subjacent bone.

The parts were brought together by sutures, water dressings applied, and the limb placed on a splint which had a foot-piece attached at a slightly obtuse angle, it extended from the popliteal region beneath the heel forwards to the toes. His after progress was most satisfactory, in six weeks the wound entirely closed, two sinuses only remaining open. The splint was removed and starch bandage applied. He was discharged from hospital on the 29th June, 1857, nine weeks after operation.

In the Winter Session of 1857-58 his case was detailed at, and the lad exhibited to, the Belfast Clinical and Pathological Society. The sinuses had closed for some months; he could lean his weight on the limb in walking, but used a stick; he had a pony, and could ride a considerable distance without pain.

He was lost sight of from that time till the autumn of 1868, when on account of the very useful limb he had it was considered of interest to exhibit him at the meeting of the Ulster Medical Society on October 5th, when the following description of his condition was given:—

He is now twenty-seven years of age, muscular and strong; has enjoyed uninterrupted good health since he left hospital; is active and fond of walking, and considers the left is stronger than the right limb; does not use a stick; in progression there is a slight halt, but not more than is frequently observed from a corn. This, to the ordinary observer, would be attributed to a slight *in-turning* of the toes of left foot, as when he walks the right foot is turned out, the left is placed straight. On examination there is considerable deposit of new tissue between the bones which were cut. The foot is slightly extended. This makes the toes and anterior part almost of proper length, but the heel is one inch and a half higher than its fellow, which he remedies by placing a wedge-shaped piece of cork wood in the heel of his boot.

The power of extension of the toes is as complete as in the opposite limb, and sensation is also perfect.

The first excision for disease of the ankle in Great Britain or Ireland was performed by Mr. Henry Hancock, in the Charing Cross Hospital, in January, 1851, and proved most satisfactory in every way. This case was the second, and, as described, the result has exceeded the most sanguine expectations, the patient not even requiring the support of the usual side irons for weak ankles. Since 1851 Mr. Hancock has never ceased to advocate this operation, and has frequently performed it. It has also been resorted to by other surgeons.

In a course of lectures delivered at the College of Surgeons, England, in 1867, on the Anatomy and Surgery of the Human Foot, Mr. Hancock[a] says:—

"I find that the ankle-joint has been excised thirty-two times for disease by British surgeons.

"That the ages of the patients operated upon varied from four to forty-two years.

"That the sexes given show the proportion of sixteen males to six females.

"That of the thirty-two patients operated upon twenty-one recovered with good useful limbs, seven died, two suffered secondary amputation and recovered, whilst the result of two was doubtful.

"The proportion of deaths consequently is a fraction above twenty-one per cent., a large proportion, I must admit, when compared with that of Syme's amputation. But it is not in reality so great when we inquire more closely into the matter; for instance, of the seven deaths four are reported to have died of consumption, of which disease one is stated to have been suffering at the time of operation; another died of the effects of syphilitic poison in the system. We can scarcely, therefore, attribute the deaths of these four consumptive patients exclusively to this operation; consequently, if we make allowance for them, we find the death-rate of average cases ten instead of twenty-one per cent., while the mortality of amputation of the leg for disease in civil surgery is twenty-six per cent."

Since that time the only case recorded is one of a *partial* excision performed by Mr. J. D. Hill, at the Royal Free Hospital.[b] It has proved successful, but not being a complete excision of the joint, it cannot be added to Mr. Hancock's table, so that the number now

[a] Lancet, 1867, Vol. ii., p. 62.

[b] Medical Times and Gazette, Vol. i., 1868.

stands thirty-three operations, "with good useful limbs" in twenty-two of them.

I was fully satisfied with the progress of the case after operation, and with the usefulness of the limb when the patient was shown to the Clinical and Pathological Society nine months after excision; but I was astonished to find such strength, capability for divers movements, and absence of deformity when I saw him in 1868. As I had been urged to amputate, and not excise, I cut the extensor tendons, as already stated, but it is worthy of being noted how completely the power of extension of the toes was restored. I should also call attention to another matter. It is held, and properly so, that the dorsalis pedis and the plantar vessels should be studiously avoided. The first named was cut, yet no appreciable lowering of temperature or other unfavourable symptoms followed.

I have frequently sought for cases which would hold out a good prospect for excision, but as yet only succeeded in this one instance. Profiting by the experience of the thirty-three resections which have been performed, if about to operate now I would avoid the tendons and dorsalis pedis artery, not as absolutely essential for the well-being of the patient, but considering it desirable to cause as little injury as possible compatible with *complete* removal of the entire diseased articulation.

ART. VII.—*Clinical Report of the Rotunda Lying-in Hospital for the Year ending 5th November*, 1869. By GEORGE JOHNSTON, M.D.; Fellow of the King and Queen's College of Physicians; President of the Dublin Obstetrical Society, and Master of the Hospital.

As the question of the great prevalence of zymotic diseases and consequent mortality in large hospitals, particularly lying-in hospitals, in comparison with the lesser mortality in extern maternity charities and small lying-in hospitals, has for some time back occupied the attention of the profession, I think this a fitting opportunity, and I consider it is incumbent on me, filling the important post of Master of the Rotunda Lying-in Hospital, to come forward and lay before you the report of our institution for the past year. From the facts therein set forth, I think I will be able to show that the accusations brought against us are incorrect.

Previously, however, to doing so, I may be allowed to repeat a

few remarks I made on a former occasion, when I asserted that "it could not be admitted that zymotic disease (puerperal fever) prevails *endemically* in hospitals *generally*," although we have no hesitation in allowing that it may appear occasionally in those which may be ill-ventilated or overcrowded, and that it was injustice to put forward the dogma that "where puerperal fever is epidemic it shows itself generally in our great maternity hospitals in the first instance." Such, I maintain, is not the case. On the contrary, how frequently have we known of it prevailing in other quarters, amongst patients confined in their own homes, and even among the affluent, when the hospital was perfectly exempt from sickness.

Nay, how much rather should we look for these diseases in the localities from whence those seeking admission emanate—in the narrow, filthy, unswept streets, the courts and alleys in too many instances reeking with the pestilential effluvia of half-putrid offal and ordure, which by imperfect sewage, or no sewage at all, allows the noxious gases escaping therefrom to pervade the overcrowded, small, unwashed, ill-ventilated apartments; their bedding, if possessed of such a luxury, saturated with filth and dirt; the unfortunate occupants frequently in a weak, emaciated state, from want, penury, starvation, and disease: in fact, everything tending to produce a poison which cannot be otherwise than injurious, and most likely to engender the malady we all have so much reason to fear. Contrast those miserable dwellings with the large, lofty, and airy wards of our institution, where perfect, thorough ventilation is observed; where the patients are not crowded together, but ample cubic space of air is allowed for each; where the strictest cleanliness in every manner is maintained, and every attention paid to their wants and well-being, by liberal diet, beef-tea, wine, &c.,—is it not reasonable to come to the conclusion that instead of the hospital being the habitat for zymotic disease, it should rather be attributed to the wretched abodes of those poor creatures who seek its shelter.

In order, therefore, to prove that what I have stated is correct, I beg leave to present to this Society an account of the deliveries, casualties, and results, as well as the particular circumstances under which they occurred, for the year ending 5th November, 1869, during which time

1,159 patients were delivered in the hospital. Of these
937 were cases of purely natural labour.
26 ,, ,, tedious do.

WARD Nº 2
34-1 x 23-9½
STORE R.ms
13-6 x 13-6
WARD Nº 3
34-1 x 23-9½
CORRIDOR
CORRIDOR
CHAPEL
35-0 x 34-0
HEAD MIDWIFE
WARD Nº 1
22-9 x 34-10½
WARD Nº 4
23-9 x 34-10½
WARD Nº 11
20-10 x 22-6
20-10 x 17-10
UPPER FLOOR
WARD Nº 7
34-1 x 23-9½
WARD Nºs
34-1 x 23-9½
CORRIDOR
CORRIDOR
CHAPEL
35-0 x 34-0
WARD Nº 8
23-9 x 36-10½
WARD Nº 6
23-9 x 36-10½
WARD Nº 12
20-10 x 22-6
20-10 x 17-10
FIRST FLOOR

47 were cases of preternatural labour, viz., 7 upper extremity and 40 lower.
81 were delivered with the forceps.
4 „ by craniotomy.
9 „ by version.
1 the perineum had to be incised, owing to extreme rigidity.
1 the os uteri had to be incised from undilatability.
27 were complicated with twins.
11 were cases of accidental hemorrhage.
1 was a case of placenta previa.
4 were cases of convulsions.
10 „ of prolapse of funis.
1 was a case of induced labour at the eighth month.
1 „ of hydatids.

Besides which we may include eight cases of mania occurring after labour, thus showing that we had the usual amount of difficulties and dangers to contend against that are liable to be met with in all large lying-in institutions.

But prior to going further into the details, and in order that you may draw a fair conclusion as to the nature and causes of the mortality, as well as to prove that the deaths which did occur in the hospital could not be attributed to any source arising from the air (or otherwise) of the institution, and also to show that no contagion existed in cases where metria or any disease of a zymotic nature presented itself, I think it but justice to the institution (so many reports having been spread as to its position and arrangements being faulty), to lay clearly before you the relative position of the different apartments in which labour took place.

Ten wards were (I say "were," as one of them, No. 11, has since October last been added to the wards for diseases of women) set apart for confinements, viz., eight in the large or principal building, and two in the auxiliary building, which stands separate from the other at a distance of seventy-two feet to the west, and in advance of it by some twenty-six feet, its back wall being thirty feet more south than the front wall of the hospital.[a] There are four large wards on each floor of the main building ; those on the upper story are numbered 1, 2, 3, and 4 respectively, Nos. 1 and 2 being opposite to one another at the west end, while Nos. 3 and 4 occupy a similar position

[a] See Dr. Phelan's letter in Medical Press and Circular for January 20, 1869, where he says "a large house adjoins it on the west side."

at the east end, and each opens on a wide corridor, running east and west 133 feet, with large windows at either end, the sashes of which are constantly more or less open. The ward at the western end on the south side of the corridor is separated from that on the east by a space of at least 35 feet occupied by the chapel; those on the northern side are separated from each other by a room fully 20 feet in width, having a large window which, with the door, is always open, and the entrance doors to each of the wards on the northern side are about 60 feet from each other; those on the southern side are the same.

The wards Nos. 5, 6, 7, and 8, on the lower corridor, are immediately under those we have been describing, and are similarly circumstanced, with the exception of the wards on the northern side, which are separated by a wide staircase with a large Venetian window, which is seldom closed. On the south side, as on the upper story, the chapel occupies the intervening space.

The two wards in the auxiliary building consist of two rooms each, one with a southern, the other with a northern aspect, and are connected by a door, which is always open. The wards on the first floor are numbered 12, those on the second floor No. 11. These last have, since the beginning of October last, been appropriated to patients labouring under uterine complaints. Each of the large wards contains seven beds, one of which is occupied by the nurse or a midwife pupil, but we seldom have more than four of the others occupied by labour patients, unless when many are coming in, and then five may be taken into the ward; so that as each room has a cubic space of 34×24 feet, and 13 in height, each patient has, on an average, 2,000 cubic feet of air; and when I mention that thorough ventilation is kept up, and extreme cleanliness observed, not only as regards the ward itself, but also in strict attention to the person of the patient, the atmosphere is quite free from any impure air; and, in fact, I may safely say that such a thing as "a loaded state of the atmosphere" does not exist.

Of course, in a large institution such as the Rotunda Lying-in Hospital, admitting, without distinction or recommendation, all who apply in their hour of trial and suffering, it is not to be wondered at that deaths do occasionally occur, and no doubt, as has been brought forward as an argument against large hospitals, the mortality has been at times apparently very great. But we must, at the same time, and before we draw a hasty conclusion, consider well the state and condition of the patients, both mentally and

physically, at the time of their admission, as well as the locality from whence they have come; and when I lay before you the tables that I have drawn up, the source and correctness of which I myself can vouch for, I flatter myself that you will agree with me that the Rotunda Lying-in Hospital should not be charged with the crime of "killing eight out of nine that could be prevented if they had been confined in their own homes or in isolated cottage hospitals."[a]

Now, in the first instance, in order to show that where metria or any disease of a zymotic nature presented itself in the wards, it was confined to that individual case, not spreading to others, either in that ward or any other part of the hospital, I draw your attention to this table (No. I.) of deaths from all causes, which, you may perceive, are entered in chronological order, giving the date of death in the first column, the No. of ward and No. of bed in the two next, and the cause of death in the last, with observations as to the state and circumstances of the case on admission.

Thus the first death that occurred was in No. 7 ward, on the 15th November, 1868; she was in bed No. 57; she was 43½ hours in labour; and was delivered with the forceps under chloroform. Peritoneal symptoms supervened, of which she died.

The next death was on November 19th, in No. 11 ward, in the auxiliary building, No. 122 bed. Her condition on admission—54¼ hours in labour; pulse, 100; tongue dry, with brown fur. She died of peritonitis. I may here remark that another patient came in to same ward at the same time, with same feverish symptoms. She was delivered by version under chloroform—the shoulder presented—and recovered.

As it would occupy too much of the valuable time of the Society, and as I consider it is not necessary for my purpose, at present, to give the details of the deaths from accidental causes, &c., I confine myself solely to those of the *zymotic* type. I have therefore drawn out a separate Table, No. II., of deaths from these diseases alone. Cases No. 1 and 2 of this table having been just mentioned, I refer you to No. 3, Table No. II. We see it took place on the 14th December, 1868, in No. 7 ward; she had been in 59 bed (a batch of patients having been confined in the ward, and discharged all well, since the death in the same ward, November 15th). She was twenty-five years of age; her second pregnancy. Showed symptoms of erysipelas when moving her from the couch

[a] See Dr. E. Kennedy's paper on Zymotic Diseases, &c., read before the Obstetrical Society, March and April, 1869.

to the bed two and a half hours after delivery. Moved to a back ward, and died eighteenth day after confinement. No other patient took the disease.

TABLE I.—*Deaths occurring in the Rotunda Lying-in Hospital from all causes, arranged in chronological order.*

No.	Date	No. of Ward	No. of Bed	Cause	
	1868.				
1	Nov. 15	7	57	Peritonitis	43½ hours in labour; chloroform; forceps; symptoms immediately after.
2	„ 19	11	122	Do.	On admission, pulse, 100; tongue dry, brown fur; 54 hours in labour.
3	„ 20	1	4	Phthisis	12 months ill.
4	„ 22	4	39	Exhaustion	Admitted pulseless from accidental hemorrhage; never rallied.
5	Dec. 14	7	59	Erysipelas	Symptoms 2½ hours after delivery.
6	„ 23	4	41	Exhaustion	Admitted pulseless from internal hemorrhage; died in one hour.
7	„ 30	2	13	Shock after operation	Bands in vagina; incised; craniotomy.
	1869.				
8	Jan. 1	12	111	Pyemia	Victim of seduction; remorse and fretting extreme.
9	Feb. 24	1	3	Peritonitis	Do. do.
10	March 20	5	47	Mental shock	Do. do. great distress of mind.
11	„ 25	7	62	Shock after operation	Induced labour by douche; died 26 hours after operation.
12	April 24	5	44	Phlebitis	Admitted with great inflammation of varicose veins of the leg and thigh; great general delicacy.
13	„ 25	8	87	Bronchitis.	3 months ill before admission.
14	June 6	1	4	Nesphritis	6 months ill before admission.
15	„ 29	8	85	Metritis	Arm presentation; waters drained off 4 hours before admission; version; placenta morbidly adherent.
16	July 28	3	23	Peritonitis	43 hours in labour before admission; had all the symptoms of peritonitis on admission.
17	Aug. 18	7	Back Ward	Mitral valve disease	Admitted in great exhaustion; died 28 hours after delivery.
18	Sept. 11	3	12	Peritonitis	Lying ill for 3 weeks before admission.
19	„ 20	2	29	Shock after operation	Very large child 10 lbs.; chloroform; forceps; sank in 39 hours.
20	„ 24	3	22	Nephritis	6 months under treatment before admission.
21	Oct. 8	3	22	Mental shock	Seduction; great mental depression.
22	„ 12	7	55	Peritonitis	10 hours in second stage; chloroform; forceps; symptoms 3 hours after.
23	„ 13	12	114	Fever, typhoid	Pulse 100, and all the symptoms of fever on admission.
24	Nov. 1	2	19	Peritonitis	In great distress from want and privation.
25	„ 4	7	59	Acute rheumatism & peritonitis	Admitted with acute arthritic rheumatism.

The next, No. 4, in Table II., took place January 1st, 1869, in No. 12 ward, No. 111 bed. She was twenty-five years of age, her first pregnancy, and died of pyemia, fifteenth day after delivery. She was admitted from a distant county, the victim of seduction; and her remorse and fretting from the moment of coming in was extreme and most distressing to witness.

No. 5, Table II., took place on February 24th, 1869, in No. 1 ward, bed No. 3. Her age was thirty-four, and she died of peritonitis. She also was the victim of seduction—distress of mind and remorse, no doubt, being the cause of the fatal illness.

No. 6, Table II., took place on April 24th, 1869, in No. 5 ward, bed No. 44; her age was thirty, her first pregnancy, and, although married, she was in great distress of mind, having been deserted by her husband and turned out by her relatives, and, as she stated to me, "she came in to die." She was greatly emaciated, suffering from distressing bronchitis, with varicose veins of leg and thigh, in a state of intense inflammation, from which phlebitis supervened, which carried her off.

Table II.—*Deaths from Zymotic Diseases occurring in the Hospital.*

No.	Date	No. of Ward	No. of Bed	Cause of Death	Conditions and Circumstances connected therewith
1	Nov. 15	7	57	Peritonitis	43½ hours in labour; chloroform; forceps.
2	„ 19	11	122	Do.	54¼ hours in labour when admitted; pulse 100; tongue dry, brown and furred.
3	Dec. 14	7	59	Erysipelas	Disease appeared 2½ hours after delivery.
4	Jan. 1	12	111	Pyemia	Victim of seduction; extreme remorse.
5	Feb. 24	1	3	Peritonitis	Case of seduction.
6	April 24	5	44	Phlebitis	Admitted in very delicate health; bronchitis; inflamed varicose veins; great mental depression.
7	July 28	3	23	Peritonitis	43 hours in first stage; tympanitis and tenderness on admission; symptoms immediately followed delivery.
8	Sept. 11	3	22	Peritonitis	Ill for 3 weeks previously.
9	Oct. 12	7	55	Do.	Tedious labour; chloroform; forceps; symptoms in 3 hours after.
10	„ 13	12	114	Typhoid	Pulse 100 on admission; fever increased.
11	Nov. 1	2	19	Peritonitis	Suffering great hardship for some time before.
12	„ 4	7	59	Acute rheumatism & peritonitis	Suffering from rheumatic arthritis on admission; result of cold caught in lodging; unmarried.

No. 7, Table II., occurred on July 28th, 1869, in No. 3 ward, 23 bed. She was twenty-four years of age, her first pregnancy, was 40 hours in labour previous to admission, when the abdomen was extremely tender, and symptoms of peritonitis, in fact, showed themselves from the commencement.

No. 8, Table II., occurred on September 11th, 1869, in No. 3 ward, 22 bed. Her age was twenty-two, third pregnancy; had been lying ill for three weeks with low fever before admission; peritoneal symptoms soon developed themselves; died sixth day.

No. 9, Table II., occurred October 12th, 1869, in No. 7 ward, bed 65 She was twenty-one years of age, her first pregnancy; ten hours in the second stage—put under chloroform, and delivered with the forceps. Symptoms showed themselves three hours after; died on the sixth day.

No. 10, Table II., occurred October 13th, 1869, in No. 12 ward, bed 114. She was twenty-eight years of age, her second pregnancy, had been suffering from illness previous to admission, when we found tongue loaded with white fur, brown and dry in centre, p. 100. Symptoms of typhoid supervened, of which she sank. No abdominal symptoms.

No. 11, Table II., occurred on November 1st, 1869, in No. 2 ward, 19 bed. She was twenty-two years of age, her first pregnancy, had all the appearance of want and starvation on admission; says she had been in the greatest distress for some time previous. Symptoms of peritonitis soon developed themselves, and she sank on the fifth day.

No. 12, Table II., occurred on November 4th, 1869, in No. 7 ward, 59 bed. She was twenty-eight years of age, her second pregnancy, unmarried; caught cold in a railway train coming up from county Meath; was lying in a lodging for a week; on admission was suffering from acute asthritic rheumatism. Peritoneal symptoms set in immediately after delivery, between which and distress of mind she sank on the fourth day.

I shall now state the total number of deliveries that took place in each separate ward during the year, with the number of deaths occurring amongst them from zymotic diseases.

Thus we find that in No. 1 ward *one death* occurred of zymotic type out of 118 deliveries.

In No. 2 ward there was also only one death of a zymotic type, whilst there were 127 deliveries in this ward within the year.

In No. 3 ward two deaths occurred, with an interval of six

weeks, during which three batches of patients had been confined in the ward, and went out well. There were 115 patients confined in this ward during the year.

In No. 4 ward there were no deaths of a zymotic nature, and we find that there were 112 patients confined in it during the year.

In No. 5 ward one death occurred out of 121 deliveries during the year.

In No. 6 ward no death of a zymotic nature occurred, although in the year there were 105 deliveries.

In No. 7 ward four deaths occurred (at intervals of one month, ten months, and one month respectively) out of 124 deliveries during the year.

In No. 8 ward there were no deaths of a zymotic type, while there were 112 deliveries, during the year.

In No. 11 ward there was one death. She had all the symptoms of fever on her when admitted. There were 105 deliveries in this ward to Oct. 22nd, when it was made a chronic ward.

In No. 12 ward two deaths occurred, at an interval of about nine and a-half months, in which there was no death, although in that time 99 patients were delivered. There were in all 120 deliveries in this ward during the year.

I think from the foregoing statements we may fairly conclude that where cases of a zymotic nature occurred, there were sufficient causes to show that such disease could not be attributed to anything endemic in the hospital; and further, that when so occurring they did not communicate contagion to any other of the inmates.

The next table I wish to draw your attention to—viz., No. III.—is framed in accordance with the Registrar-General's Report, and, in my humble opinion, is most valuable, showing, as it does, that sickness of a zymotic type may prevail outside, even to a considerable extent, yet the hospital remain comparatively free, if not totally exempt from it.

In this table you perceive I have conformed to the Registrar-General's division of the city, within the municipal boundary, into seven districts, viz., 3, North, and 4, South, of the river Liffey.

I have taken the trouble of ascertaining the locality from whence each patient confined in the hospital came. I have divided them into the districts as arranged by the Registrar-General, and entered them accordingly in the first column; in the second column I give the number of deaths, if any, that took place from zymotic diseases, out of the foregoing number; and in the third column I

give the number of deaths that occurred in the same district, within the same period, from zymotic diseases. I should mention that 100 cases, in addition to those tabulated, came from the country, so could not be entered in the column as coming from any of the Metropolitan districts, yet I place the deaths which occurred out of them in No. 2, North City District, that being the district in which the hospital is located.

TABLE III.—*Number of Patients admitted and delivered in the Rotunda Lying-in Hospital from each Municipal District; the Number of Deaths from Zymotic Diseases that occurred amongst them; and the Number of Deaths from the same causes occurring in that District in the same period.*

	NORTH CITY DISTRICT									SOUTH CITY DISTRICT												
	No. 1			No. 2			No. 3			No. 1			No. 2			No. 3			No. 4		
	Admitted into Hospital	Died in Hospital from Zymotic Disease	Deaths in District from Zymotic Diseases	Admitted into Hospital	Died in Hospital from Zymotic Disease	Deaths in District from Zymotic Diseases	Admitted into Hospital	Died in Hospital from Zymotic Disease	Deaths in District from Zymotic Diseases	Admitted into Hospital	Died in Hospital from Zymotic Disease	Deaths in District from Zymotic Diseases	Admitted into Hospital	Died in Hospital from Zymotic Disease	Deaths in District from Zymotic Diseases	Admitted into Hospital	Died in Hospital from Zymotic Disease	Deaths in District from Zymotic Diseases	Admitted into Hospital	Died in Hospital from Zymotic Disease	Deaths in District from Zymotic Diseases
For 1 mth. 7th Nov. to 5 Dec., inclusive	16	–	32	27	–	20	7	1	20	3	–	25	7	–	16	8	–	16	13	1	27
to 2 Jan.	24	–	29	19	1	10	5	–	15	2	–	13	4	–	9	8	–	18	12	1	12
to 6 Feb.	17	–	39	36	–	5	12	–	26	7	–	16	12	–	13	16	–	24	17	–	17
to 6 Mar.	20	1	21	18	–	13	8	–	22	3	–	18	10	–	8	7	–	16	19	–	4
to 3 April	23	–	25	22	–	7	3	–	11	1	–	19	9	–	16	10	–	16	15	–	7
to 1 May	19	1	25	22	–	6	1	–	11	7	–	23	16	–	7	11	–	9	13	–	9
to 5 June	34	–	16	28	–	9	2	–	16	1	–	22	12	–	11	8	–	13	14	–	17
to 3 July	18	–	12	24	–	4	3	–	13	2	–	17	4	–	4	11	–	9	9	–	4
to 7 Aug.	33	–	21	40	–	12	7	–	18	1	–	33	8	–	9	10	1	17	11	–	7
to 4 Sept.	18	–	13	30	–	24	1	–	26	4	–	25	20	–	11	6	–	14	17	–	7
to 2 Oct.	15	–	21	23	–	15	5	–	38	5	–	28	13	1	23	1	–	19	7	–	13
to 6 Nov.	23	4	18	35	–	20	3	–	36	–	–	23	6	–	26	6	–	26	12	–	12
	260	6	272	324	1	145	57	1	252	36	–	262	121	1	153	102	1	197	159	2	136

Thus, by referring to the Table, we may see that from the 7th November, 1868, to December 5th, 1868, inclusive, a period of

one month in round numbers, sixteen patients were admitted and delivered in the hospital from No. 1 District, North City, all recovered; although by the Registrar-General's Report we find thirty-two deaths recorded for the same district and same period, from zymotic diseases.

Twenty-seven patients were admitted and delivered from No. 2, North City district, within the same period; all recovered. Yet from the Registrar-General's Report, twenty deaths from zymotic disease took place in the same district in the month.

From No. 3, North City district, there were seven admissions and deliveries in the month, out of which one died from zymotic disease. We find at the same time, by the Registrar-General's Report, that twenty deaths occurred in that district.

From No. 1 South City district we had 3 admissions and deliveries; all recovered. We find that during the same period by the Registrar-General's Report that 25 deaths from zymotic diseases are recorded.

From No. 2 South City district there were 7 admissions and deliveries; all recovered. At the same time 16 deaths from zymotic diseases are recorded in the Registrar-General's Report.

From No. 3 South City district there were 8 admissions and deliveries; all recovered. At the same time 16 deaths from zymotic diseases occurred in the district.

From No. 4 South City district there were 13 admitted and delivered in the hospital, out of which 1 died; we find that 27 deaths from zymotic diseases are recorded in the Registrar-General's Report for the month.

But as it would encroach too much on the time of the Society, and be rather fatiguing to you to go into all the details of each month, perhaps it will be sufficient to give you the account of the sum total in each district for the year ending 5th November, 1869. Thus from No. 1 North City district the there were 260 admissions and deliveries, out of which we had 6 deaths from zymotic diseases, while we find by the Registrar-General's Report there were 272 deaths from the same causes in this same district and period.

From No. 2 North City district we had 324 admissions and deliveries, out of which 1 died from zymotic disease. Still by the Registrar-General's Report 145 deaths from zymotic diseases are recorded for the same district.

From No. 3 North City district there were 57 admissions and

deliveries, out of which there was 1 death from zymotic disease, at the same time there are 252 deaths from zymotic disease recorded for that district.

From No. 1 South City district there were 36 admissions and deliveries, all recovered, but we find 262 deaths from zymotic diseases occurred in the same district during the year.

From No. 2 South City district there were 121 admissions and deliveries, out of which 1 death took place from zymotic disease, while by the report we find 153 deaths from same causes recorded as having occurred in the district.

From No. 3 South City district there were 102 admissions and deliveries. 1 death from zymotic disease occurred. We find at the same time there were 197 deaths from the same causes in the district.

From No. 4 South City district there were 159 admissions and deliveries, out of which 2 deaths from zymotic disease occurred. We find at the same time 136 deaths recorded, from same causes, in the Registrar-General's Report; and, finally, viewing it in the abstract, we observe there were 1,059 patients admitted and delivered in the hospital, from the seven districts in the city, besides 100 cases admitted and delivered from the country, out of all of which 12 died from zymotic diseases, while we see from the Registrar-General's Report there were 1,417 deaths, from the same causes, in the same period in these seven districts.

Comparing this great amount of mortality in the various districts outside, with the small amount of deaths that have taken place in the hospital, and more particularly when we take into consideration all the peculiar circumstances and conditions under which the different fatal cases were admitted,—we may fairly come to the conclusion that we have no reason to regret keeping the doors of the Rotunda Lying-in Hospital open, feeling fully assured that those admitted have in it a far safer asylum than in their own homes, and fully as safe as if confined in cottage hospitals.

I have thus endeavoured to record fully and faithfully the clinical history of the hospital during the past twelve months, and in so doing I have been particular to supply all such information as may help to show whether, or how far, metria was engendered in the wards of the hospital, or communicated from one patient to another.

ART. VIII.—*On the Treatment of Incontinence of Urine in Childhood and Youth by Collodion.* By SIR D. J. CORRIGAN, Bart., Physician in Ordinary to the Queen in Ireland.

I PRESUME only to offer in this short paper a contribution to our list of adjuvants in the treatment of incontinence of urine during sleep, in childhood and youth. In the list of maladies not fatal there is, perhaps, not another so productive of misery and anguish as this affection.

The child of five years of age feels acutely his misfortune when he involuntarily wets his bed, and fears to meet his nurse in the morning with his face of shame.

The schoolboy who is subject to it carries about with him through his day of study and recreation a load of misery.

The climax, however, of anguish is, when the boy, grown into the young man, enters civil life, college, or the army, and dreads with fear and horror the discovery of his misfortune, though in no way his fault.

To such a degree has this terror risen that it has been sufficient to deter a young man from persevering in a course of life that otherwise presented all that he could desire.

The treatment of this affection may be divided under two heads: the first, the constitutional treatment, being that conducted on the principle of strengthening the whole system, in the hope of including in it the erring organ: the second, that of dealing directly with the organ in fault.

With the first, or that of constitutional treatment, I do not deal at all in the present paper.

The second head of treatment, or that of dealing with the organ at fault, has two subdivisions. The first having reference to medicines supposed to exert a specific action on the bladder, such as blisters, belladonna, copaiba, and to preparations of iron acting with a double effect, local and constitutional, and the second relating altogether to mechanical means of treatment, all difficult or impossible to be carried out. It is with this last I now propose to deal.

Various mechanical means of treatment have been recommended, for instance, a perineal truss. It is scarcely necessary to observe that its application is attended with insurmountable difficulties, the

impossibility of exactly adjusting its pressure, its interference with rest, and the exposure attendant on using it.

Another means has been proposed, and it is said in some cases successfully used, of applying along the urethra on the outside and under surface of the penis a portion of bougie, which is kept in its place by bands of adhesive plaster applied round the penis, with sufficient pressure to prevent the urine's forcing its way along the urethra.

It is scarcely necessary to observe that the use of such mechanism is utterly impracticable in nearly all cases of this miserable affliction in childhood, boyhood, and early manhood.

The young sufferer naturally turns to the use of mechanical means for relief, and we have on record instances in which the poor child, under terror of shame and discovery, has tied a ligature of cord around the penis, and under the influence of the same feelings has concealed his sufferings until gangrene has set it; and Gross relates a case in which a brass curtain ring was used, which became imbedded in the inflamed corpora cavernosa, and was removed years afterwards in fragments, along with a calculus lying in a pouch behind it.

Long time since I tried to close the opening of the prepuce with adhesive plaster, or court plaster, and in one case, that of a young man of twenty years of age, who had entered the army, and who was intelligent and careful, the result was favourable, but there were great difficulties about it. The application took time, and could not be carried into successful effect with boys and children.

The mechanical treatment to which I now wish to draw attention is the treatment by collodion. It is most easy of application, occupies scarcely a minute, and can be carried out at school, college, or elsewhere, in perfect privacy.

All that is necessary is, while the prepuce slightly curved up is held with the left hand, to smear over the little cup thus formed by the extremity of the prepuce with collodion by means of a small camel's hair pencil or blunt end of a penholder. Almost as fast as applied the collodion solidifies. In contracting it draws closely together the edges of the prepuce, and thus the exit for the escaping urine is closed.

A boy of eleven years of age has, after one lesson, been able to use the collodion, and has used it every night carefully and diligently, so anxious has he been to cure himself of what he considered a disgrace. A fortnight's use is sometimes sufficient for

the cure. A relapse is easily dealt with. A solution of gutta-percha in chloroform would seem at first sight to be equally applicable, but it is not. The solution of gutta-percha is much longer in hardening, and it possesses no contractile powers.

When the child or youth desires to pass water the little wedge or cap of collodion is easily removed with the finger nail.

When I first used this collodion application, my expectation was, that the bladder would act so forcibly against it as to cause sudden pain, and oblige the patient to jump at once out of bed and quickly remove the collodion, and that he should then repeat the application before returning to sleep.

I was agreeably disappointed. There was no pain; no awaking; but on rising in the morning the prepuce was found slightly distended with urine, and the collodion was removed without difficulty.

This unexpected result would lead us to infer that the action of the muscular contractile fibres of the bladder has little to do in causing the nocturnal incontinence of urine in childhood, and that the escape of the fluid is rather due to want of apposition in the sides of the canal of the urethra, or to a feeble state of the circular fibres which are supposed to constitute the sphincter of the neck of the bladder.

Acting on this view I have recommended the child's bed to be made in such form as that while preserving the usual slope for head and shoulders, the rest of the bed shall gently rise as an inclined plane, from hips to feet, so as to allow the urine in the bladder to gravitate towards the fundus, rather than towards the trigone.

In all the instances in which I have used the collodion application the prepuce has been, as, I believe, is nearly always the case in children and boys, projecting over the glans, or in a state that may be designated as natural phimosis. But I do not anticipate any difficulty in using the same application in cases where the corona glandis might be exposed, as the collodion would, I should expect, act equally well over the orifice of the urethra as over the opening in the prepuce.

It may be presumption, but I cannot conclude without expressing a strong opinion against one part of the management of these cases, which is very frequently practised through the best motives on the part of mothers and nurses, and sanctioned by the recommendations of some medical authorities—the practice of awaking the child at stated intervals to pass urine.

I have never seen any benefit whatever from it, and I think I

have, on the contrary, always seen aggravation of the affection from it, not unreasonably to be explained, on the principle that such practice is really training the bladder to empty itself at stated periods, when the object is that it should be trained to retain its contents.

ART. IX.—*Death from Inhalation of Chlorine Gas.* By CHARLES A. CAMERON, M.D., Professor of Hygiene in the Royal College of Surgeons in Ireland; Analyst to the City of Dublin, &c.

IN Taylor's comprehensive work on "Medical Jurisprudence," no cases of death from the inhalation of chlorine gas are given. It is, indeed, stated that the French chemist, Pelletier, died from the effects of chlorine gas taken into his body—whether in the gaseous condition or dissolved in water I know not. The following particulars relative to a fatal case of poisoning—if I may use the term—from chlorine may not prove uninteresting, and perhaps it may prove to be the first case of the kind on record.

A small ship, lying in the river Liffey, took in a cargo composed partly of salt cake (sulphate of soda), partly of chloride of lime. The former was stowed away in bulk in the hold; the latter was stored in casks, which were placed on the salt cake. On the morning of the 18th January, 1870, one of three sailors who had passed the previous night in the forecastle, was found dead on the floor of that cabin. His companions were comatose; but under treatment on board the vessel, and subsequently at Sir Patrick Dun's Hospital, they recovered.

On the evening of the 18th January I was requested by the coroner, Dr. Nicholas C. Whyte, to assist his medical adviser, Dr. R. W. Egan, in investigating the cause of the death of this sailor. We ascertained that two of the casks of chloride of lime had burst and their contents had been scattered, and several of the staves were charred. This accident might have occurred either from the chloride of lime having been over-heated by the salt cake—which is often shipped in so hot a state that it can hardly be handled—or from water having got into the casks.

The forecastle was rather a large one for a small schooner—its space being 940 cubic feet. During the night there was fuel consumed in a small stove, but the products of combustion must have readily escaped through the opening—which was four square feet—in the roof. Those facts disprove the statements which were

made, to the effect that the sailors were poisoned by the gases generated in the stove.

The body of the deceased was conveyed to the place where Dr. Egan performs his autopsies. The surface of the corpse was well washed (ammonia being added to the water), so as to remove from it every trace of chlorine, and the *post mortem* examination was then commenced. The following were the principal morbid appearances.

Face.—Pale, not livid, pupils dilated; white froth oozing from the mouth and nostrils; the lips blanched and somewhat swollen; the tongue and gums blanched, the latter appearing as if they had been completely bleached. The epithelium was soft, and in some spots gone in the case of the lips, tongue, gums, and palate.

Neck.—Deep veins very full.

Lungs.—Congested, not very dark; the bronchial tubes full of frothy sanguineous matter; the trachea inflamed. The lungs otherwise normal. All the internal venous system full.

Heart.—Remarkably fine and healthy; left side quite empty; right side full of dark blood.

Stomach.—Healthy, as are all the other abdominal viscera. This organ was perfectly empty.

Head.—Veins of scalp full; scalp and dura mater uncommonly adherent; surface of the brain and its membrane only slightly congested; sinuses full; substance of brain very firm, and in all respects healthy.

During the examination a tube was introduced into the trachea, and pressure having been applied to the chest, a bloody and very frothy liquid, possessing a slight odour of chlorine, was evolved through the tube. On cutting into the ventricles of the brain a very strong and unmistakable odour of chlorine was evolved. No odour of the gas was perceived in the abdominal cavity.

Here is what probably occurred on the morning of the 18th January:—The deceased was awakened from his sleep by the irritative action of the chlorine on his respiratory organs; he struggled out of his berth, became sick (vomit was found on the floor, and not a particle of food in the stomach), got a spasm of the glottis and died from apnea. Perhaps violent and rapid inflammation of the larynx might have occurred. Ammoniacal gas has been known to produce death by the intense inflammatory action set up immediately after its inhalation.

The autopsy was made thirty hours after the body was discovered; and it is not a little remarkable that after so long a period the chlorine should be so easily recognized in the brain.

ART. X.—*Richmond Hospital Records.* By WILLIAM STOKES, Jun.; Surgeon to the Richmond Surgical Hospital; Lecturer on Theoretical and Operative Surgery, Carmichael School of Medicine; Fellow of the Royal Medico-Chirurgical Society of London, &c., &c.

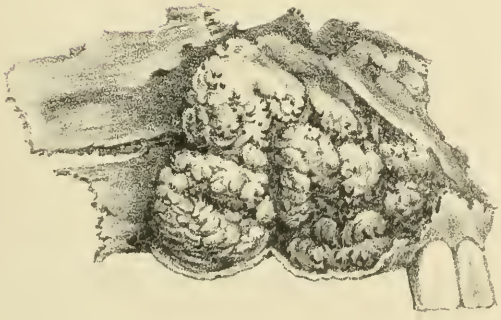
I. EXCISION OF THE UPPER JAW.
II. AMPUTATION AT THE ANKLE-JOINT.
III. PRIMARY AMPUTATION AT THE ANKLE-JOINT.
IV. AMPUTATION OF THE FORE ARM.
V. AMPUTATION OF THE THIGH.
VI. AMPUTATION THROUGH THE TARSUS.
VII. AMPUTATION OF THE PENIS.
VIII. SUB-PERIOSTEAL EXCISION OF THE ELBOW-JOINT.
IX. OVARIOTOMY.
X. TRACHEOTOMY.
XI. BUROW'S PLASTIC OPERATION.
XII. TABULAR VIEW OF THIRTY-THREE CASES OF FRACTURE.

A RECORD, as briefly given as the particulars of the cases will admit of, and for the treatment of which I deemed the surgical operations above indicated, necessary, will not I trust, be uninteresting to those engaged in surgical practice.

1. *Excision of the Upper Jaw.*—James M., aged fifty, was admitted into my wards in the Richmond Surgical Hospital on the 1st of March, 1868, having been recommended to me by my eminent colleague, Dr. Lyons.

The patient stated that about a year before the date of his admission into hospital, a small ulceration appeared at the junction of the soft and hard palates. This ulceration, which presented all the external characters of epithelioma, was removed by operation in September, 1867. After some time, however, the disease returned, and spread slowly, both anteriorly and posteriorly. It never extended, however, beyond the mesial line. About a month before the patient's admission into hospital the disease appeared externally, on the cheek namely, and from this opening there was a constant oozing of clear fluid, which, as the opening was in the immediate vicinity of Steno's duct, was most probably saliva. The general health of the patient was excellent, and there was only one small gland which was perceptibly enlarged, and which was situated immediately below the angle of the jaw. The epithelial ulceration,

From a Photograph taken nineteen days after the Operation



Mr Stokes on Excision of the Upper Jaw

which was very irregular, extended considerably, both in front of and behind the junction of the soft and hard palate. There was occasional, but very slight hemorrhage from the ulceration. The patient slept well, and his appetite was excellent.

Having regard as well to the external characters of the disease as to its clinical history, it was tolerably evident that the case was one of epithelioma, which had originated in the muco-periosteum of the hard palate, and which subsequently involved the bone to a considerable extent. Although, doubtless, the case could not be considered as typical for the operation of resection, permanent benefit, as Professor Syme observes, being in all cases more confidently to be expected from the operation, when the consistence of the tumour is essentially firm, yet, considering the comparatively limited extent of the disease, the case was one that demanded prompt operative interference.

On March 4th, the patient was brought into the operating theatre, and placed seated in a strong high backed chair. His arms and legs having been carefully secured by bandages to the chair, chloroform was then administered. When the patient was brought fully under its influence, I commenced by making an incision from the inner angle of the eye downwards and outwards to the inner edge of the salivary fistula. The incision was then carried horizontally inwards towards the mesial line near the septum of the nose, and finally brought vertically downwards, a little to the right of the central line through the red border of the lip. The inner flap, or that next the nose was dissected carefully backwards towards the left side, and then the two superior maxillary bones were separated by Charrière's long bone forceps, one limb of this powerful instrument being placed along the floor of the nose, and the other in the mouth. The separation of these bones was effected by this admirable instrument with the utmost facility. The posterior flap was then dissected backwards towards the arch of the zyzoma. At the junction of the superior maxillary bone with the malar, the separation was effectually accomplished by one of Langenbeck's small resection saws. When this was done the saw was then carried upwards and inwards, below the infra orbital ridge, up to the junction of the nasal bone with the nasal process of the superior maxillary bone. Great care was taken not to interfere with the floor of the orbit. The necessary division of the osseous structures having been now accomplished, the separated bone was seized with a lion forceps, and without much difficulty was removed. Other

portions of bone which were found to be diseased, were carefully removed by straight and rectangular gouges, and the horizontal plate of the palate bone was also removed by strong forceps; portion of the soft palate on the right side was also excised by strong curved scissors. The hemorrhage was not at all so great as was anticipated. Only one vessel had to be ligatured. Any hemorrhage from others was effectually arrested by the actual cautery. No plugs of lint or any dressings were placed in the cavity. The edges of the wound were then brought together by numerous points of metallic wire suture.

As regards the progress of this case, little that is specially remarkable is to be observed. No secondary hemorrhage occurred. Two days after the operation, symptoms of erysipelas of the head were developed commencing on the bridge of the nose. However, under the use of antimonials and mild purgatives, &c., they subsided. The wound in the face healed with surprising rapidity. Four of the sutures were removed on the third day, and the remainder on the fifth. The case progressed in every way most favourably. On the tenth day after the operation the following note is in my case book:—"The patient is free from all pain and uneasiness. Eats, drinks, and sleeps well. His articulation is becoming more distinct every day. There is no sign of the re-formation of a salivary fistula."

On March 23rd, exactly nineteen days after the operation, the patient was photographed by Mr. Forster, of Westmoreland-street, and on the following day he returned to the country. The accompanying lithograph is taken from the photograph.

Amputation at the Ankle-Joint.—Amputation at the ankle-joint, as first suggested and practised by Professor Syme, has long and justly been considered one of the greatest modern improvements in practical operative surgery. The following case adds another to the long list of those which have shown so unquestionably the great importance of this procedure.

Bridget C., aged twenty-three, a native of the south of Ireland, was admitted into the Richmond Surgical Hospital on the 27th March, 1868. The patient is of a delicate strumous temperament, and, from long-continued suffering, and many sleepless nights, had the care-worn and anxious expression of face, seen so invariably in those who are the subjects of protracted and painful disease. The patient states that about two years ago, without any assignable

cause, the joint became affected. She does not remember ever having got any blow on or near the joint, nor does she remember ever having even sprained the ankle. About twelve months after, the joint first became affected, the pain and swelling in it became so aggravated as to render walking or standing no longer possible. She was then recommended by her friends to consult a bone-setter of some local celebrity, who informed her that in his opinion the bones of the ankle-joint were displaced, and that an attempt should be made at once to restore them to their natural situation. Accordingly the joint was subjected to what, from the description, must have been very violent extension and twisting, in order to effect the reduction of the supposed luxation. After this mistaken and violent treatment, the previously existing symptoms became much aggravated, the pain especially, which never allowed her a single night's rest. Any attempt to walk or use the joint was now wholly out of the question. Things remained in this way for several months, and then as the symptoms were evidently not diminishing in intensity, she determined to seek for surgical advice in Dublin.

On examination, the enlargement, which was very considerable, was found principally over the astragalus. There was exquisite tenderness over the joint, especially at its inner side. The slightest motion of the joint gave extreme pain. At night she suffered much from shooting and stinging pains, which gave her much distress. There were no sinuses or any evidence of abscess formations. The anterior portion of the foot was healthy. There were no hectical symptoms, cough, or diarrhea, and her appetite was good. Looking at the history of the case, the urgency of the symptoms, and the length of time they had lasted, it was clear the case was one demanding prompt operative interference, and I contemplated one of two procedures, namely, either resection of the ankle-joint, or amputation in the manner first recommended and practised by Professor Syme. Looking, however, at the debilitated condition of the patient, and the strong probability of the disease not being sufficiently localized in any particular part of the tarsus, circumstances which would militate strongly against a successful resection of the joint, I deemed amputation preferable, and accordingly proposed the operation to the patient, who willingly acceded to it.

On the 2nd of April, the operation was performed. In arresting the hemorrhage, the method of torsion to which I was anxious to give a fair trial, did not prove at all as satisfactory as I would have

desired. Very considerable difficulty was experienced, especially in arresting the bleeding from an interosseous artery, which eventually had to be secured by a ligature. On examination of the foot, the disease was found to be more extensive than from the previous symptoms and appearances I had anticipated. All the bones of the tarsus were more or less diseased, the morbid process being specially evident in the astragalus, the greater portion of which was completely softened and diseased. A very remarkable condition was found on the articular surface of the tibia ; namely, an adventitious ridge or bony prominence, which extended horizontally across the centre of the articular surface of the bone, and which seemed to have become developed in order to replace the body of the astragalus, which, from disease, had become softened and flattened. The cartilage of incrustation on the articular surfaces of the tibia and astragalus was eroded and ulcerated, but the carious process in both the tibia and fibula appeared to be extremely limited, and when the section of the bones was made, the osseous structures appeared perfectly normal.

In making some clinical remarks suggested by this case to the class of the hospital, I could not avoid congratulating myself that amputation and not resection had been resorted to, for, notwithstanding the arguments so ably and forcibly put by Mr. Hancock in favour of resection in cases of caries of the ankle-joint, there can be no doubt that although the operation of resection is indicated before all others in many cases of injuries of the ankle, such as, for example, gunshot injuries, as shown by the remarkable results obtained by Professor Langenbeck during the Schleswig-Holstein war of 1864, and in some cases of disease obviously originating from injury; yet that, in caries of the ankle-joint, in the ordinary acceptation of the term, the morbid process is seldom if ever so localized as to justify resection. In such cases, therefore, amputation at the ankle-joint is in every respect the most satisfactory and most scientific procedure that can be had recourse to. It is unnecessary to give the daily progress of this case. On May 2nd, exactly one month after the operation, it is noticed in my case book that " during the past week the case has progressed without a bad symptom; wound is nearly healed; promises to be a most successful result." A fortnight after this date, the wound had completely healed.

Primary Amputation at the Ankle-Joint—Patrick Masterson,

aged twenty-five, by occupation a railway porter, was admitted into the Richmond Surgical Hospital, on the 30th of last April, suffering from a severe crush of the foot which he received half an hour previously, in consequence of the wheel of an engine having passed over it. He was standing near the rail as a locomotive was passing by, when a bolt struck him pushing his foot under the wheel. There was a very extensive and deep lacerated wound, not only on the dorsum but also on the sole of the foot, and all the metatarsal bones, with the exception of that of the great toe, were crushed. Owing to the extensive and deep wound on the sole of the foot, I deemed it unadvisable to perform the medio-tarsal amputation, but determined, after consultation with my colleague, Professor R. W. Smith, to perform Syme's amputation at the ankle-joint. The operation was accordingly performed at 10 a.m. on the day of his admission. The first three days after the operation everything went on favourably, but on the fourth there was a decided change for the worse, the pulse rose to 112, and there was a violent erysipelatous inflammation extending from the wound up towards the knee. There was also the commencement of the formation of a small slough on the inner side of the leg, about two and a half inches above the wound.

May 6th.—Slough much increased, also the inflammation, which has assumed all the characters of phlegmonoid erysipelas, has extended above the knee.

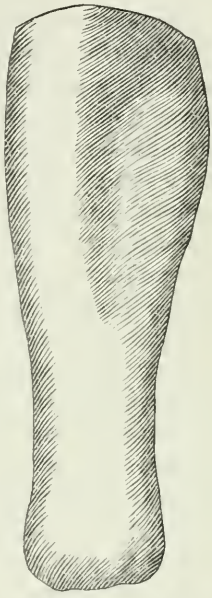
May 7th.—Pulse 116; at 9 30 a.m. the resident clinical clerk, Mr. Briscoe, found that a very sharp hemorrhage had taken place; the limb, in fact, was bathed in blood, and the patient, in a state of extreme collapse and weakness from the loss of blood. Mr. Briscoe and Mr. Vesey arrested the hemorrhage, which came from the flap, by elevating the limb, and making firm pressure on the femoral artery. I was immediately sent for, and on my arrival found that the bleeding had entirely ceased. Nothing further was then done, except to administer freely diffusible stimulants.

May 8th.—Pulse 116; much stronger. Patient in every respect improved. Inflammation not so violent.

May 9th.—There was another smart hemorrhage to-day from the flap. Portion of it appears as if it were about to slough away, which eventually it did.

May 12th.—Much better; pulse 100. From this date the patient continued to improve rapidly. The portion of the flap that sloughed was replaced by healthy granulations. These soon cicatrized, and on 17th June the patient was able to go about on

crutches, and when he left the hospital the wound had completely healed. After an interval of four months, I saw the patient again, and a better shaped and more useful stump I have never had an opportunity of seeing. The accompanying woodcut is taken from a cast of the limb.



This case is of greater importance than the preceding one in proving the great merits of Syme's amputation, as it shows that even when the unfortunate accident of partial sloughing of the flap occurs, the surgeon need not consequently at all despair of the patient obtaining eventually a useful and well-formed stump.

Amputation of the Forearm.—Mary O'B., aged thirty-seven, was admitted into the Richmond Surgical Hospital, on March 7th, under my care, suffering from a large encysted synovial tumour connected with the flexor tendons of the forearm. The tumour was extremely painful, and from this circumstance, as well as from its size, rendered the limb useless to the patient, whose avocation was that of a workwoman. Several methods of treatment, having for their object the absorption of the fluid contents of the tumour, such as pressure, iodine applications, blistering, &c., were had recourse to, but without producing any effect whatever on the

tumour. After consultation, it was determined to puncture the tumour with a cataract needle, and in this way to give exit to a small amount of the fluid. This was done, and for some days the case went on very favourably, but the improvement was only temporary, for after this, symptoms of inflammation in the tumour supervened, which no antiphlogistic measures that were adopted succeeded in allaying. Suppuration then took place, and there was no course open to us, then, but to lay the tumour freely open. After this the most violent symptoms followed. Large abscesses formed in the hand and forearm, which had to be freely opened as they formed. There was also complete and permanent contraction of the fingers. As the inflammation and suppuration were increasing in intensity and extent, and rapidly exhausting the patient, and as the limb was now, from the complete contraction of the fingers, and the matting together of the muscles of the forearm, quite useless to the patient, I recommended amputation, and performed the antero-posterior flap operation about a hand's breadth below the elbow-joint, on May 6th. No difficulties attended the operation; the hemorrhage was chiefly arrested by torsion, and the wound dressed subsequently by Lister's antiseptic method. In this case union did not take place by first intention, but the amount of suppuration which followed was, I am bound to say, extremely small, and the healing process went on with great rapidity.

Amputation of the Thigh.—No topic in surgery has of late years more actively engaged the attention of surgeons than that of determining the method of amputation of the thigh, which is fraught with least danger to the patient, and affords the most satisfactory results, not only as regards the appearance, but also the form of stump which can be best adapted to the subsequent application of mechanical appliances. The fact that many of such operative procedures have each of them advocates, is a sufficient proof that the opinion of surgeons is still to a great extent unsettled as to the best mode of thigh amputation. There can be little doubt that of the comparatively novel procedures the "rectangular method," suggested first by the late Mr. T. Pridgin Teale, and which in the Dublin School has found such a warm advocate in my friend, Mr. Croly, is one which, perhaps more than any other, has found favour among surgeons. This results from the supposed advantages of this operation, which are doubtless familiar to my

readers, and which it is therefore unnecessary to enumerate. However, objections have been raised to this method which are not by any means devoid of foundation. These are—1st, the great extent of cut surface that the operation necessitates; 2nd, the longitudinal division of muscular tissue; and 3rd, the high division of the bone. The amputations of Spence and of Carden get rid to a great extent of these defects. Of this latter operation, the amputation by a long anterior skin flap, notwithstanding the favourable accounts Mr. Carden gives of his results, I cannot think highly. The operation which seems to me to get rid best of two of the supposed defects in Mr. Teale's amputation—viz., the extensive division of muscular tissue, and the high division of the bone—is the method which has been suggested by Mr. Wharton.

It is, I believe, an undisputed fact that, at all events in amputation of the thigh, the nearer to the trunk the bone is divided the greater is the risk to the patient. Dieffenbach, in one of his lectures on amputation of the thigh, pithily expresses this in the sentence, "Zollweise steigt die gefahr," which freely rendered in English means, "the danger increases by the inch." Any modification therefore of the method of the rectangular amputation, which has for its object the saving of bone, and which, in all other respects, affords equally good results as in the method recommended originally by Mr. Teale, must obviously be an improvement. The modification may be given in Mr. Wharton's words:—"Let the measurements for Teale's amputation be accurately taken and mapped out, but instead of dissecting the bone—I am now specially referring to the thigh at the site directed by him—let it be sawn at the situation corresponding to the inferior termination of *his* posterior flaps, and let the superior boundary of the anterior flap be limited by the same plan. The saving of bone thus effected will be considerable, and therefore tend directly to diminish the mortality of thigh amputations. According to this method the anterior flap will be shorter by a fourth than Teale's, and there will be no posterior flap." As this proposal appeared to me to be based on sound surgical principles, I determined that I would give it a trial the first opportunity that occurred.

Shortly after this, J. Hoey, aged thirty-four years, by occupation a sailor, was admitted into the Richmond Surgical Hospital on account of very extensive lupoid ulceration of the right leg. It commenced about eight years ago after a fall over a rope, against

which the lower part of the leg struck, which he sustained while running along the deck of the vessel during a dark night. Shortly after this, ulceration appeared in the situation where the injury had been received, and commenced to spread extensively.

For some time after his admission into hospital the case was actively treated, both locally and constitutionally, and very considerable improvement took place. So much so, that he was making arrangements to return home, when suddenly a sloughing process supervened, having many of the characters of hospital gangrene. This spread rapidly, engaging the dorsum of the foot, and laying bare the tendons, muscles, and arteries. From the latter, which became ulcerated and opened, very sharp hemorrhage occurred, which was with much difficulty controlled; indeed, it was not satisfactorily arrested until I passed acupressure needles under the vessels, and made pressure on the arteries by passing loops of wire across the needles. The "third method of acupressure" was adopted, and proved in every respect perfectly satisfactory. As the disease spread close to the knee-joint, I did not think it advisable to amputate below the articulation, and I was also deterred from doing so from the fact of the disease attacking the stump, and I did not think either that I could get sufficient healthy integument to cover the broad cut surface of the tibia in front. Behind, I admit, I might possibly have obtained a sufficiency, but I was deterred from making the attempt by a very strong objection which I have to long posterior flaps taken from the calf of the leg. All things considered, I deemed it preferable to amputate above, but as near to the knee-joint as I could, and I have not, I must allow, been disappointed in the result, whether as regards situation of the amputation or the method adopted.

It is unnecessary to enter into any of the minute details, either as regards the operation or the subsequent progress of the case. The former was performed without any deviation from the proposed modification of Teale's amputation, and of the latter, one mishap has to be recorded (though, in truth, it might be adduced as an argument in favour of the operation to those who object to the modification on the grounds of there not being a sufficiency of flap), and that was, that four or five days after the operation a small portion of the anterior flap sloughed away; but, notwithstanding this, there appears to me to be an ample covering of soft parts on the face of the stump. Now, what are the objections which have been urged by surgeons against the modification?

They are six in number, and may be briefly enumerated as follows:—

1.—The cicatrix being too near the face of the stump.

2.—Tension of soft parts on the face of stump.

3.—Insufficiency of covering.

4.—That, owing to retraction of the tissues, the posterior surface of the bone projects, and is denuded of soft tissues.

5.—Liability of cicatrix to become adherent to the immediately subjacent bone.

6.—Increased risk of pyemia.

As regards the first of these objections, I would remark that it appears to me that instead of the cicatrix being too near the face of the stump, the chances are that, owing to the retraction of the posterior tissues, it will be further away from the face of the stump than in the operation as originally suggested by Mr. Teale. When the posterior tissues are divided they immediately retract, and the retraction does not cease at the time of the operation, but continues during the process of healing,—a fact recorded long since by Alanson—and consequently the cicatrix is drawn high up on the posterior surface of the limb. So much, then, for the cicatrix being too near the face of the stump.

The second objection is the liability to tension of the soft parts on the face of stump. This, it has been said, exists in Hoey's case. It may, I confess, be there to a certain extent; but I do not think that it will in any way interfere subsequently with the usefulness of the stump. As regards this point, too, it must be remembered that he had a certain loss of tissue from sloughing of the anterior edge of the flap. Had that not taken place there can be no doubt we should not have had the amount of tension, such as it is, which has been observed.

The result of my case shows also that the third objection, viz., insufficiency of covering, is not a valid one.

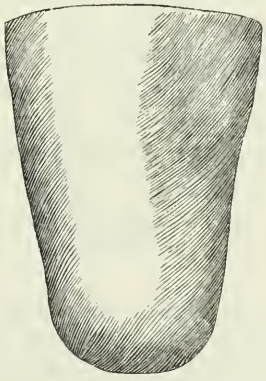
The fourth objection is one which Mr. T. Pridgin Teale, the son of the originator of the rectangular flap operation, has made in a letter which he published in the *British Medical Journal* for Nov. 7, 1868. He observes, "if, however, the posterior tissues be divided opposite the point selected for sawing through the bone, they immediately retract to a point higher up, and thus have the posterior surface of the bone projecting and denuded of soft tissues." At first sight this appears a grave objection; but we need not fear this so much when we reflect "that," as Mr. Lister observes, "it

fortunately happens that the bone lies forward among the muscles, so that even its posterior surface is considerably anterior in position to the longitudinal axis of the limb." The tendency is always for the bone to come forward; and if a proof of this were wanting, we have only to bear in mind the fact that in cases where ulceration of the flap takes place, it is in the great majority of cases through the anterior and not through the posterior or lateral position of the stump that the bone protrudes.

Projection and denudation, therefore, of the bone posteriorly need not, I think, be apprehended.

The remaining objections to the modification—viz., the liability of the cicatrix to become adherent to the immediately subjacent bone, and the increased risk to pyemia, need not, obviously, be dwelt upon.

Such, then, are the advantages of this modification of Teale's amputation, the chief objections which have been raised against it, and the way in which it appears to me they may be best answered. Although I have performed the operation frequently on the dead subject, I have only performed it once on the living, and the result can be judged of by a reference to the accompanying woodcut, taken from a cast of the stump.



I should be sorry to advocate this operation in preference to all other methods of amputation of the thigh. This I do not wish to do, for the result of my experience so far, in amputation cases, is in favour of the circular method, for in this, the subsequent chances of pyemia are, I think, diminished, as there is less exposure of soft tissues, and the veins are divided vertically, at right angles to their continuity, and not obliquely, as they are in all flap

amputations, which latter must render these vessels more liable to take on inflammatory action, from the wounds in them being necessarily so much greater in extent.

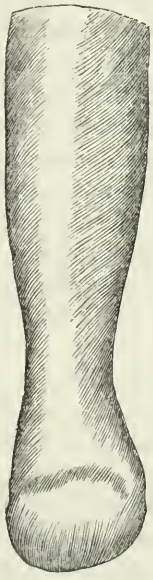
Amputation through the Tarsus.—The cases suitable for the performance of Chopart's amputation are very limited, but among them few appear to be better adapted for it than the one which is the subject of the following brief record:—

J. B., aged seventy, a hale old man, who, until recently, had always enjoyed good health, was admitted into the Richmond Surgical Hospital, under my care, in the early part of September, 1868. He stated that about a year previous to his admission into hospital he observed a wart between the fourth and fifth toes of his right foot. For some time it gave him no distress, but after three months it began, from its increasing size, to give him annoyance, and he consulted a local practitioner, who recommended, and applied a powerful caustic to it. This treatment, however, had not the desired effect of destroying the disease, for shortly after ulceration at the situation of the wart appeared, and spread rapidly to the adjacent toes. For several months he was under surgical treatment in the country, and various caustics were applied, but without effectually checking the progress of the disease.

At the time of his admission into the Richmond Hospital the ulceration presented all the external characters of epithelial cancer, and involved the four lesser toes. Small, but frequently repeated hemorrhages took place from the ulceration, which, doubtless, to a great extent, were the cause of the pale anemic appearance the patient had on his admission into hospital. He also suffered occasionally from very severe shooting pains at the situation of the ulceration.

In this case Lisfranc's amputation might, doubtless, have been performed; but adopting a course which I have frequently heard recommended by my colleague, Mr. Adams, of the importance of having a healthy joint between a disease such as we had to deal with in this case, and the situation selected for amputation, I deemed the medio-tarsal operation of Chopart preferable, and accordingly performed it. I operated in the ordinary manner, and met with no difficulties either during or subsequent to the procedure. After the operation was over, and the edges of the wound brought together by numerous points of interrupted silver

wire sutures, I dressed the wound by the antiseptic method, and in doing so, followed as closely as I could the directions given by Professor Lister. Although I cannot by any means endorse all that has been said in reference to this method of dressing wounds, the result in this case was, in truth, very remarkable, for the amount of suppuration which took place during the healing of the wound was almost *nil*. It is unnecessary to give a daily report of the progress of this case. Nothing could have been more perfectly satisfactory. The accompanying woodcut, taken from a drawing of a cast of the stump, will give a better idea than a written description of its appearance.



Amputation of the Penis.—In amputating the penis close to the pubis in cases of extensive cancerous and other diseases necessitating its removal, the operating surgeon has not unfrequently two great difficulties to contend with, which are, first, to prevent the retraction of the stump into the abdomen; and secondly, to arrest the hemorrhage which occasionally supervenes after the retraction has taken place. I have witnessed a truly eminent surgeon all but baffled in his attempt to arrest the bleeding from the face of a stump on its retraction after amputation at the pubis, and the patient faint from loss of blood. Under such

unfortunate circumstances the difficulties in seizing and ligaturing a bleeding vessel cannot, in truth, be exaggerated. To obviate these difficulties the plan I wish to advocate is one which I first saw practised for the purpose of arresting hemorrhage from the cord, in cases requiring castration, in the Meath Hospital, by my eminent friend, Mr. Porter. It may, I think, well be termed acupressure *en masse*. It consists simply in passing, previous to the amputation, a long acupressure pin through the body of the penis, taking care not to transfix the urethra by keeping well in front of it, and then passing over it a loop of strong iron or silver wire. It is, in truth, nothing but a modification of the third method of acupressure. The principal vessels, which are necessarily divided in the amputation, can thus, previous to the operation, be effectually constricted between the acupressure pin behind and the loop of wire in front. The cases of castration I have above alluded to were, when the vessels in the cord were dealt with in this manner, almost bloodless operations, and, in truth, the same may now be said of amputation of the penis, if this method, which I can strongly recommend, be adopted, having employed it not only in cases requiring castration, but in those of amputation of the penis, when that organ had to be removed close to the pubis, and in all with uniform success.

Case I.—Peter M., aged fifty-six, was admitted into No. 6 Ward in the Richmond Hospital, under my care, in February, 1869, suffering from extensive epithelial cancer of the penis. He stated that a warty growth appeared about two years previously on the glans, that it ulcerated and commenced to spread, which it continued doing until the whole organ became engaged. There was a profuse discharge of thin, sanious, and peculiarly fetid matter, so much so as to contaminate the air of the whole ward. From the ulceration also, there was frequently small hemorrhages, which, however, could always be easily arrested. Strange to say, notwithstanding the extensive and truly frightful mass of disease that existed in this case, there was little evidence of glandular contamination in the groins or abdomen. Having no doubt in my mind as to the cancerous nature of the disease, a view which microscopical examination subsequently verified, I recommended amputation, which, in consequence of the very extensive amount of disease, had to be performed close to the pubis. The patient having been brought fully under the influence of chloroform,

I commenced by passing one of the long pins used for the first method of acupressure through the body of the penis, in front of the urethra. I thus effectually constricted all the tissues in front of the pin by passing a loop of strong silver wire across, and fixed the wire by giving it a double twist round the handle of the pin I then removed the organ with a small catlin, and on doing so found there was no bleeding, except from one small vessel beside the urethra, which was not compressed by the pin and wire. The hemorrhage from this vessel was easily arrested by torsion. The urethra was then slit up to the distance of an eighth of an inch, and fixed by two points of intercepted suture to the integument. A catheter was then introduced into the bladder, and fixed by worsted threads to the acupressure pin. On the second day after the operation the acupressure pin was removed. The catheter, however, was kept in the bladder for some days longer. Carbolic acid dressings were applied during the healing of the wound. The case went on in every respect most satisfactorily. Three weeks after the operation, the wound having completely healed, the patient left the hospital, being able to pass water in a full, strong, and uninterrupted stream. Six months after the operation I had an opportunity of seeing the patient, and found him in excellent health, and no evidence whatever existed of any return of the disease.

Case II.—John D., aged sixty, was admitted into No. 7 Ward in the Richmond Hospital, under my care, on the 17th May, 1869, suffering from cancer of the penis. The case presented many features of analogy to the preceding one as regards the amount of disease present, the length of time it had lasted, and the sanious and fetid discharge, which continually oozed from the ulceration. It differed, however, from it in this, that there was obviously a large amount of glandular contamination. The operation of amputation therefore, as was previously explained to the patient, had only for its object the giving of temporary relief. The amputation was performed precisely in the same manner as in the preceding case. The wound healed readily, and the patient returned to the country. Seven months after the operation I heard of this patient from a medical gentleman in the county Mayo, where the patient resides, and he informed me that until quite recently there was no reappearance of the disease at the site of the operation.

An objection has been made to this method, which is the danger of introducing and leaving for a considerable time in venous erectile tissue, a foreign body, such as an acupressure pin; for that by doing so the surgeon runs the risk of inducing phlebitis. No special operative interference with veins is, doubtless, devoid of risk, but at the same time, when one reflects upon the remarkable tolerance of all living tissues for metallic bodies, and that, so far as I am aware, no instance of phlebitis following acupressure has yet been recorded, the surgeon need not, I think, be apprehensive of inducing that disease by the employment of acupressure in the operation of amputation of the penis.

Sub-periosteal Excision of the Elbow-Joint.—James C., aged thirty, was admitted into the Richmond Surgical Hospital on the 18th of last October. He stated that about fourteen months previously, while playing at ball, his foot slipped and he fell. The right elbow was struck with great violence on the ground. Soon after this the joint swelled, and became very painful. From the description given by the patient it was very evident that the case was one of acute arthritis of the elbow-joint. The inflammation never entirely subsided, and six months after the accident, small abscesses formed over the posterior aspect of the joint. At the time of his admission into hospital the joint had become quite anchylosed, and in a very awkward position for the patient, being almost quite extended. The joint presented all the well-known characters of the so-called strumous disease of the elbow, viz., the globular form, the soft parts puffy and elastic, and numerous sinuses which, on being probed, led down to softened diseased bone, &c., &c. Any attempt at flexion was attended with great pain. The patient was otherwise in good health. There was no pulmonary affection, no enlarged lymphatic glands or scars in the neck, nor any evidence of a strumous tendency except the affection of the elbow. This being the case, I deemed it in every respect suitable for resection, and accordingly I performed the operation. In doing so, I adopted the sub-periosteal method, the steps and advantages to be derived from which, I have already drawn the attention of my surgical brethren in the pages of this Journal, as well as in those of the *British Medical Journal.*

After the removal by Langenbeck's resection saws of the articulating surfaces of the bones, which were found extensively diseased, a piece of oiled lint was placed in the wound to serve as

a vent for the pus, and the edges of wound brought together by numerous points of metallic interrupted suture. The limb, in a semi-flexed position, was then encased in a gypsom bandage. The following day I cut an oval window of the length of the wound in the gypsom bandage, and after covering all with a solution of Dammar resin, to prevent the action of the water from softening the gypsom, the limb was placed in the continual bath, such as I have described in the paper in this Journal which I have already alluded to. The arm was kept in the bath for three weeks, at the end of which time, the wound being nearly healed, it was taken out, and a splint applied on the anterior surface of the arm. It is unnecessary to detail the daily progress of this case. A month after the operation the wound was completely healed, and in six weeks all the sinuses. The limb is still (Dec. 6th) very stiff, but has already some power of flexion and extension, and there is every reasonable prospect of the patient getting eventually excellent use of the arm.

Ovariotomy.—Maria L., a married woman, aged fifty-seven, was admitted into the Richmond Surgical Hospital on the 14th of September, 1868, having for some time previous to this been under the care of my colleague, Dr. Lyons, in the Whitworth Hospital. She first observed enlargement of the abdomen in the Spring of 1864. On her admission into the Richmond Hospital the tumour had reached an enormous size, the girth at the umbilicus being fifty-three inches. The catamenia had always been regular, and she had enjoyed excellent health in every way until the tumour began to grow. About two months previous to her admission into hospital, she suffered from considerable pain in the upper portion of the tumour, and at this situation a peritoneal friction sound was distinctly audible, even to the patient herself. The attack, however, soon subsided. At the time of her admission to the Richmond Hospital she was greatly emaciated, and suffering much from abdominal distension. In other respects the patient was in good health. At this time Dr. Beatty and Dr. Byrne saw the case in consultation with Dr. Lyons and me, and the opinion arrived at was that, notwithstanding the emaciated condition of the patient, the case was one for which the operation of ovariotomy was indicated. On September 15th, the patient having been previously brought under the influence of chloroform, was brought into the operating theatre. I commenced by making a longitudinal incision

four inches in length, midway between the umbilicus and pubis, and a careful dissection down to the peritoneum was then made. That structure was then raised off the sac of the cyst by small hooks, and carefully divided in the direction of the original incision. On opening the peritoneum some clear straw-coloured fluid escaped, and then the shining white sac of the cyst was brought distinctly into view. I then thrust a large trochar, with caoutchouc tubing attached, into the interior of the cyst, and the fluid contents were allowed to flow through the tubing into a large bucket underneath the operating table. Several quarts of a clear tenacious honey-like fluid were evacuated from the first cyst. This being emptied, without removing the trochar, I thrust it into the second cyst, and the fluid contents of this were found to differ completely from the first, being of a dark brown chocolate colour, opaque, and very much thicker, and more tenacious. Twenty-seven quarts of fluid were evacuated from these two cysts. The tumour having now sufficiently collapsed I passed my hand gently between the sac and the peritoneum, to determine whether any adhesions existed. These I found principally at the upper portion of the tumour, and broke them down without any difficulty. I then drew slowly and carefully out through the incision, and the pedicle I found narrow and of considerable length. The folding hinge-clamp, which Dr. Beatty had kindly lent me for this operation, was then applied, the handles removed, and the pedicle divided with a scalpel. The edges of the wound were then carefully brought together above and below the clamp by several points of silver suture, great care being taken to include the peritoneum. Strips of adhesive plaster were placed across the wound, dry lint dressing applied, and the patient immediately removed to the adjacent ward, and placed in a bed previously warmed with hot jars. The patient soon after this rallied, and took some warm brandy, and expressed herself greatly relieved and gratified at the operation having been concluded. At this time (11.40 a.m.) her pulse was 112, and soon after she fell into a quiet sleep.

1 p.m.—Still asleep; pulse 104. Shortly after this she awoke, and had a little burnt brandy, which she appeared to like. I saw her again at 4.15 p.m. with Mr. Fleming. Everything was apparently progressing favourably, no head symptoms, pain in the abdomen, sweating, or vomiting. The surface of the body maintained an equable warm temperature; pulse 120. I visited her again at 9.45 p.m.; all was going on well. She took some iced brandy and chicken broth with relish.

Second Day, 1 a.m.—Complained of great restlessness, with pain over the pubis, accompained with slight retching, which was checked, and the pain alleviated by the administration of a very small quantity of burnt brandy; pulse 112; some difficulty in breathing, which was relieved by raising her shoulders. 3 a.m.—Pulse 130; had slept a little; does not complain of any pain or uneasiness. At 11.30 a change for the worse occurred. A cold, clammy perspiration appeared; the pulse rose and became very weak; she had also frequent retching, and great prostration. Nutritive enemata was now administered frequently, and had the effect of making her rally somewhat, but this improvement did not last. She gradually sunk, and at 5.20 p.m. she died. The autopsy revealed the usual evidences of extensive peritonitis; the intestines were much distended with flatus; some fluid was also found in the cavity of the abdomen resembling that contained in the cyst, and lymph was found effused over the surface of the liver. A fibrous tumour about the size of a small orange was found attached to the fundus of the uterus.

Tracheotomy.—The following case is one of very considerable surgical interest, and illustrates, in a remarkable manner, the great practical value of the early performance of tracheotomy in certain cases of œdema of the glottis.

Ann Fullam, aged three years, was admitted into the Richmond Hospital, on Tuesday evening, the 19th June, 1868, suffering from very great dyspnea. Her mother stated that at five o'clock in the evening, the child attempted to take a drink from a kettle in which there was a quantity of boiling water. Nothing occurred in consequence of this to attract attention, until after the lapse of about two hours, when the breathing becoming short, quick, and stridulous, she was brought to hospital. The following note was taken on the patient's admission:—"The lips were found to have been scalded, and there was a mark down the chin as if produced by a stream of boiling water. The soft palate was whitish, but the tongue was not burned. There was great dyspnea, with considerable stridor, and a crowing noise resembling that in croup, when she cried. The face was not much congested, but had a glazed, red, shining appearance. She could scarcely speak, uttering now and then a few syllables, which were hoarse and rough."

The case was seen by me shortly after 10 p.m., and notwith-

standing the prompt measures which were had recourse to in order to relieve the dyspnea, consisting of hot fomentations, poultices to the throat, local depletion, and the active administration of antimonials and mercury internally, the symptoms, as the night advanced, grew worse.

The dyspnea, which came on in paroxysms of great intensity, became much aggravated, being more frequent and more violent, so that at times the patient was in imminent danger of suffocation. After midnight the symptoms became so alarming, that I determined that tracheotomy was the only resource left in order to save the patient's life. The operation was accordingly performed above the isthmus of the thyroid, and was greatly facilitated by the tracheotomy hook of Langenbeck. The tube which was introduced was of a size fully adapted for an adult, and to this circumstance I attribute largely the success of the operation.

On the completion of the operation immediate relief ensued, and the child slept soundly the whole night, the tube being occasionally cleared of mucus by means of a feather or a bent probe, with a little French wadding tied on it.

May 20th.—Progressing very favourably; does not suffer any inconvenience from the tube; is very drowsy, and sleeps a great deal. In order to arouse her from what appeared a sort of semi-comatose condition, we administered a small quantity of green tea, which was suggested by Mr. Adams. It had the desired effect. She took also plenty of nourishment, consisting of chicken-broth, egg-flip, wine, &c. The inner tube only required to be taken out once to be cleaned.

21st.—She slept well during the night. The tube required to be cleaned once, and during the day twice. The mucus is very tenacious, and neither a feather nor a probe armed with lint has any effect in bringing it up. Sometimes cleaning the inner tube seems to have no effect in giving relief, the mucus in the trachea being driven up to the outer tube, and is only got rid of by coughing. In the evening there was a slight attack of vomiting, which relieved her greatly.

22nd.—The child sat up this morning. Does not suffer apparently from any inconvenience in wearing the tube. The bowels were well moved.

23rd.—The tube was taken out this morning, but the glottis not being free enough for the child to breathe through, the tube was replaced. The child is cheerful and lively, and eats heartily.

24th.—The tube was taken out to-day, and as it was found she could breathe slightly through the mouth, the tube was left out. Large quantities of viscid mucus are frequently expelled through the wound.

25th.—Patient breathes much more freely through the mouth to-day, but cannot yet speak. Small doses of carb. ammoniæ in syrup of orange flowers were administered.

28th.—Patient attempting to speak. Wound in the trachea granulating and closing quickly.

29th.—Articulation much more distinct. Child very anxious to get up.

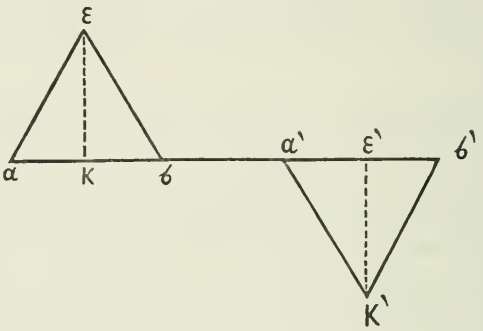
30th.—The opening in the trachea is now so small as to be scarcely perceptible. Patient speaks distinctly. Appetite is excellent. Patient is cheerful and happy, and respiration perfectly natural.

Burow's Plastic Operation.—P. Delany, aged sixty, was admitted into the Richmond Hospital on May 20th, 1868. The patient stated that about fourteen years ago he received the prick of a needle in the cheek, in the situation of the morbid growth, for the removal of which he had come into hospital. Shortly after getting this apparently trivial injury he perceived a small wart about the size of a grain of rice form exactly in the situation where he got the prick of the needle. The wart remained stationary for a very considerable length of time, after which it seemed to get loosened, and the patient then picked it off with one of his nails. Another then formed in the same situation, and ran precisely the same course. Things remained in this way for several years, the disease being apparently quite localized, and the general health of the patient remaining in every respect perfectly unimpaired. About six months previous to the patient's admission to the Richmond Hospital, the warty growth began to increase in size, and when he came under my care the tumour was fully an inch in length and half an inch in breadth. There was no evidence of any similar disease elsewhere.

The operation which I performed in this case was one originally devised and performed by M. Burow, a distinguished Polish surgeon, and the steps of which I learned from having seen the operation performed some years ago by M. Arlt, the eminent Professor of Ophthalmology in the Vienna University. It appears to me to be specially applicable to operations necessitating

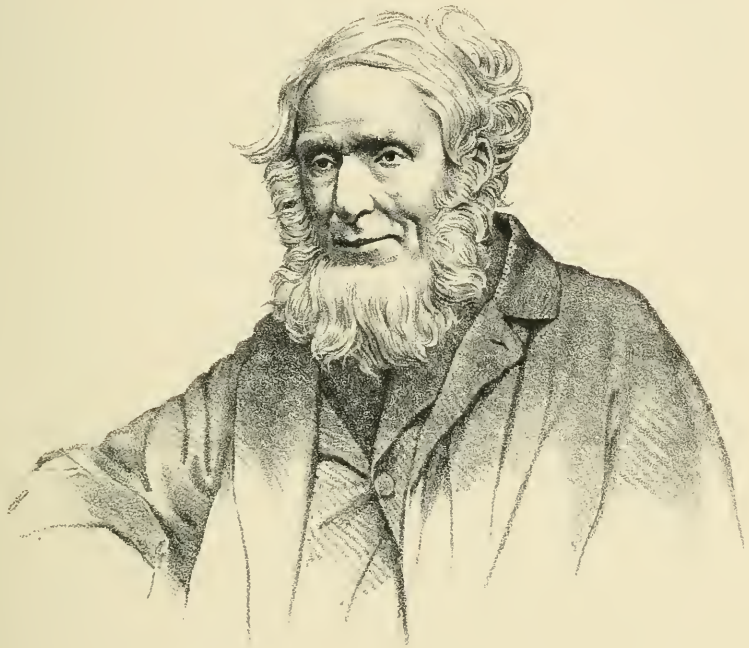
the removal of morbid growths on the face, as subsequently the occurrence of a permanent cicatricial deformity is not liable to occur.

The patient having been brought fully under the influence of chloroform, the tumour was encircled by three incisions in the form of a triangle (*a e b*), the apex of the triangle (*e*) being above the tumour, and the base (*a b*) below. The tumour and portion of the integument to which it was attached were then carefully excised. The incision (*a b*) was then carried outwards until its entire length (*a* b^1) was three times that of the base of the triangle (*a e b*). These particulars of the operation will doubtless be better understood by a reference to the annexed wood-cut.



The next step consisted in making the outer third of this horizontal incision (a^1 b^1) the base of a second triangle (a^1 k^1 b^1) in every respect equal to the original one (*a e b*), when the tumour was removed. The integument within the incision, constituting the second triangle (a^1 k^1 b^1), was then carefully dissected off. There were then two raw triangular spaces to be covered in by sound integument, one which had been the seat of the epithelial cancer, and the other in which the integument was perfectly healthy. This was easily effected by dissecting off with great care the triangular flaps (*a* a^1 k^1) and (*e b* b^1) from the subjacent structures. This having been done, no difficulty whatever was experienced in bringing the points *a* and *b* together by one suture, and the points a^1 and b^1 by another. In this way the triangular spaces were completely filled up, without any chance of a broad cicatrix, which would necessarily have been left had either of them been left to fill up by granulations.

The sutures were left undisturbed for three days, after which



Result obtained after Burows Operation
(From a Photograph)

they were removed. The greater portion of the wound had united by first intention; indeed all, except the points corresponding to the apices of the two triangles. In order to promote union at these points, I inserted two "entomologist" pins, and by a figure of eight suture brought the ununited edges of the wound into close apposition. Nothing could have been more entirely satisfactory than the result of this admirably conceived and extremely ingenious plastic operation. As this is the first time this operation has been performed in this country, the particulars of it and its most successful termination cannot but prove interesting to those who practice this all-important and thoroughly practical department of Operative Surgery.

Richmond Surgical Hospital.

Table *giving Statistics of Thirty-three Cases of Fracture treated in* Mr. Stokes's *Wards, From February 1st to July 31st*, 1868.

No.	Nature and Cause of Injury	Sex and Age	Method of Treatment	Result	Remarks
1	Compound comminuted fracture of the lower jaw; from a blow of the foot-board of a swing—left side	Male, aged 11.	Iron sutures applied round teeth on each side of fracture; external support with Hide's prepared felt and a four-tailed bandage.	Recovery.	The wire sutures removed fifteen days after the accident.
2	Fracture of femur at centre of shaft, from kick of a horse.	Male, aged 28.	Liston's splint.	Recovery.	
3	Comminuted fracture of tibia and fibula; from a fall while intoxicated.	Male, aged 46, coachman.	Gypsum bandage, afterwards Dupuytren's splint on back of leg.	Recovery.	After the gypsum bandage was taken off ten days after its application, a partial displacement of the lower extremity of tibia forwards was found; this was reduced, and Dupuytren's splint applied on back of leg, and a thick compress placed over the situation where the bone had become displaced.
4	Colles' fracture of the radius; from a fall.	Female, aged 25.	Gordon's splint.	Recovery.	No deformity.
5	Fractured ribs; fall.	Male, aged 34.	Broad strips of adhesive plaster round injured side of thorax, and flannel bandage.	Recovery.	
6	Extra capsular imparted fracture of neck of thigh bone; fall.	Male, aged 68.	At first long extension splint, removed after three weeks.	Recovery.	Shortening half an inch.
7	Fracture of fibula; from a blow of a piece of "pig" iron.	Male, aged 25.	Gypsum bandage.	Recovery.	

8	Intra capsular fracture of the neck of thigh bone; from a fall down a flight of stairs.	Female, aged 60.	Pillow under knees.		
9	Fracture of the femur at junction of upper and middle thirds of the bone; fall off a car. Femur had been twice fractured before; the first time thirty years ago; treated by the late Mr. Cusack, in Stevens' Hospital. The second time, four years ago—Mr. Fleming, who treated the second fracture, believed it opened into the knee joint.	Male, aged 50.	American permanent extension method; two broad strips of adhesive plaster on each side of the leg, united by a loop below the foot; over a pulley a "deep-line" cord was placed, one end attached to the loop of plaster, and to the other a 7 lb. iron weight was fixed. Lower part of the bed was raised so that the counter extension was effected by the weight of the patient.		This method was adopted in consequence of the patient having had a large ulcer over the external malleolus on the injured side As this required daily dressing, it was considered that the necessary frequent removal of the long extension splint would be attended with great inconvenience, and accordingly this mode of extension was adopted, and an oval window in the plaster having been made, the dressings were daily applied without in any way disturbing the limb.
10	Fracture of the fibula; from being tripped up and falling.	Male, aged 31.	Gypsom bandage.	Recovery.	No deformity.
11	Fracture of external condyle of left humerus, a door closing on it.	Male, aged 10.	Pasteboard splints.	Recovery.	No deformity.
12	Fracture of tibia about centre of bone; from the blow of the shaft of a cart.	Male, aged 50.	Box splint.		
13	Fracture of both clavicles, comminuted of left; transverse fracture of sternum opposite cartilage of fourth rib; fractures of all the ribs on left side, and of the three last ribs on right side; fracture of both bones of right fore-arm, and comminuted fracture of right ala of ilium. Caused by being crushed by the wheel of an engine for working the sawing machinery in a timber yard.	Male, aged 40.	—	Death.	

TABLE—*continued.*

No.	Nature and Cause of Injury	Sex and Age	Method of Treatment	Result	Remarks
14	Fracture of left humerus, at centre of the shaft.	Male, aged 19.	American humerus extension splint.	Recovery.	No deformity.
15	Comminuted fracture of the scapula and clavicle—clavicle fractured in six fragments; fracture of the first, second, third, seventh, eighth, and ninth ribs; from a fall down a flight of stairs.	Male, aged 55.	—	Death.	
16	Fracture of clavicle, apparently between conoid and trapezoid ligaments; little or no deformity; from a blow on the shoulder by the buffer of a railway carriage.	Male, aged 24.	Conical pad in the axilla.	Recovery.	Slight deformity.
17	Fracture of clavicle at its greatest curvature; from a fall.	Female, aged 15.	Conical pad in the axilla.	Recovery.	Slight deformity.
18	Fracture of metacarpal bone of thumb; from a blow of a strong stick.	Male, aged 13.	Two narrow straight splints along the dorsal and palmar surface of the thumb.	Recovery.	No deformity.
19	Fracture of first phalanx of thumb; from a blow against an iron railing while patient was running very rapidly.	Male, aged 12.	Gypsum bandage.	Recovery.	
20	Fracture of tibia and fibula; from a horse falling on patient.	Male, aged —	Two lateral metal splints.	Recovery.	
21	Fracture of tibia.	Male, aged 14.	Glue bandage.	Recovery.	
22	Fracture of tibia and fibula; from a fall.	Male, aged 42.	Two side splints.	Recovery.	
23	Compound fracture of metacarpal bone of thumb; from a blow received by the fall of a barrel. Violent inflammation of hand and fore-arm.	Male, aged —	Continual bath at first until inflammation had subsided; then a straight splint along front of fore-arm and hand.	Recovery.	

24	Pott's fracture of the fibula, with fracture and displacement inwards of inner malleolus; from a fall while wrestling.	Male, aged 38.	Dupuytren's splint, subsequently gypsum bandage.	Recovery.
25	Fracture of the radius three and a half inches above its carpal extremity; from a fall.	Male, aged 50.	Pistol splint.	Recovery.
26	Fracture of the lower jaw, near the angle; from a blow.	Male, aged 38.	Splint made of Hide's prepared felt.	Recovery.
27	Collis's fracture of the radius; from a fall.	Female, aged 60.	Pistol splint.	Recovery.
28	Fracture of the humerus at surgical neck. Patient was knocked down by a blow on the head, and on falling the humerus struck against the edge of a stone pavement and was fractured.	Male, aged 45.	American humerus extension splint, afterwards pasteboard splint.	Recovery.
29	Fracture of tibia; from a fall	Male, aged 7.	Two lateral pasteboard splints.	Recovery.
30	Fracture of ribs, with laceration of lung hæmothorax, and extensive emphysema; from a fall off a cart.	Male, aged 70.	Broad strips of adhesive plaster, opium, diffusible stimulants, &c., &c.	Death.
31	Fracture, with depression of portion of right parietal bone, extensive effusion of blood between calvarium and dura mater, from laceration of middle meningeal artery, also considerable subarachnoid effusion corresponding to seat of injury.	Male, aged 26.	—	Death.
32	Fracture of the fibula about two inches above external malleolus; from a fall while wrestling.	Male, aged 22.	Box splint.	Recovery.
33	Fracture of tibia and fibula about one inch above the ankle-joint.	Male, aged 23.	Two side zinc splints.	Recovery.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

RECENT WORKS ON DISEASES OF THE EYE

(*Continued from page* 161.)

1. *Treatise on the Diseases of the Eye; including the Anatomy of the Organ.* By Carl Stellwag von Carion, M.D. Translated by Charles E. Hackley, M.D., and D. B. Roosa, M.D. London: Robert Hardwicke. 8vo, pp. 774.

2. *A Manual of the Diseases of the Eye.* By C. Macnamara, Surgeon to the Calcutta Ophthalmic Hospital. London: John Churchill and Sons. 1868. Pp. 571.

3. *A Treatise on the Diseases of the Eye.* By J. Soelberg Wells, Professor of Ophthalmology in King's College, London. London: John Churchill and Sons. 1869. 8vo, pp. 741.

4. *An Improved Method of Extraction of Cataract.* By J. R. Wolfe, M.D. London: John Churchill and Sons.

5. *The Theory of Ocular Defects and of Spectacles.* Translated from the German of Dr. Hermann Scheffler, by R. B. Carter, F.R.C.S. London: Longmans, Green and Co.

6. *Diseases and Injuries of the Eye, with Medical and Surgical Treatment.* By George Lawson, F.R.C.S., Surgeon to the Royal London Ophthalmic Hospital, Moorfields, and Assistant-Surgeon to the Middlesex Hospital. London: Henry Renshaw, 1869.

The consistency of the lens, the presence, size and consistency of a nucleus will as a rule indicate whether the cataract should be removed by discission and absorption or by extraction. The very ancient operation of removing soft cataracts by suction has recently been revived and improved upon by Mr. Pridgin Teale of Leeds, and has found some advocates, especially at the Royal London

Ophthalmic Hospital, "where it has been employed with marked success" (Wells). We ourselves rather agree with Stellwag when he says:—"The operation may well be considered as a *linear extraction with artificial hindrances.* It appears void of all practical value, even if with it there be more attention paid to divide the capsule extensively, and thus avoiding a secondary cataract." The 14th volume of the *Archiv für Ophthalmologie* contains a most valuable historical notice of this operation by the late Dr. Sichel, who concluded that the operation was practised in Persia before the ninth century. This conclusion he founded on documentary evidence, especially on an Arabic MS. on eye diseases, by Isa-ben-Ali, an oculist who flourished in Bagdad in the ninth century. In this MS. is figured the hollow cataract needle which was then in use, a *fac-simile* of which, together with the original explanatory text, is given in the *Archiv.* This essay of Sichel's possesses, besides its great intrinsic merits, the additional interest that its proof-sheets were revised by the author on his death-bed shortly before his decease. All who knew Sichel or his writings must feel what a loss ophthalmology has sustained in his death. An accomplished scholar, familiar with Oriental and European languages, and possessing an intimate acquaintance with Greek and Roman literature, cultivating natural history, archæology, &c., and profoundly versed in his own speciality, he was a man of unusual knowledge and varied acquirements. He was by birth a German, and early settled in Paris by the advice of his teacher, Jaeger, and was we believe the first to establish in France special instruction in ophthalmology; he was a skilled practitioner and a judicious and trustworthy adviser. Constant and unceasing were his labours, loving work and study for their own sake; and so strong were his habits of life that they ceased only with death. In a letter to v. Graefe a very short time before his death, he wrote:—"So long as my head is clear I am all right, for I can work, and that was ever the happiness of my life."

Very great and general attention has been paid of late years to the extraction of cataract, and various old operations have been revived and new ones adopted with a view to facilitate the exit of the cataract and to obviate or diminish the dangers attendant upon flap extraction. The accidents during that operation and the evil results succeeding it are truthfully and forcibly dwelt upon by Stellwag, who states that

"Even under the most favourable circumstances it has been found

that, of fifteen eyes operated on, one is destroyed by suppuration, about three are severely affected with inflammation, especially iritis, and require a second operation for the restoration of sight; but the rest receive sight by the extraction, even if only to a limited extent, and subsequently require, at most, the removal of the posterior capsule, which is not unfrequently cloudy."

With respect to the corneal suppuration after flap extraction Wells remarks:—

"It has been long a keenly-debated question whether the suppuration commences in the iris and passes thence to the cornea, or whether it originates in the latter, and extends secondarily to the iris and ciliary body. Von Graefe maintains the latter view. According to him, the iritis which occurs at this early stage is propagated secondary, whereas that which comes on at a later period is primary or simple iritis."

Stellwag inclines to the same opinion. The principal methods of removing the lens bodily, which have been brought before the public during the last few years, are a straight incision in the cornea followed by a partial iridectomy and the removal of the lens by means of a spoon introduced behind the lens (linear extraction, Waldau and v. Graefe); a straight incision into the corneo-sclerotic junction, complete iridectomy and removal of lens by means of a modified Waldau's spoon (traction extraction of Bowman and Critchett); a straight incision in the sclerotic and corneo-sclerotic junction, complete iridectomy and expression of lens by simple pressure (v. Graefe's new modified linear extraction); flap extraction preceded by iridectomy (Mooren); flap extraction succeeded by iridectomy (Jacobson); flap extraction of the cataract in its capsule (Beer), (Richter), and with iridectomy (Pagenstecher). These have now been reduced to v. Graefe's new operation, and the old flap extraction with or without or preceded by iridectomy. The good old flap extraction finds still many defenders, as it yields undoubtedly, when successful, the most perfect results, results which, as far as vision and cosmetic effects are concerned, are infinitely superior to those achieved by any other operation. Stellwag's chapter, though containing ample and valuable information, is somewhat discursive, and has left a feeling of doubt on our minds as to which operation he holds in highest esteem. He seems to favour the old flap extraction, but admits the advisability and indeed the necessity in many cases of an iridectomy either previous to or at the time of operation. The following would appear to

indicate his preference for the corneo-sclerotic incision and v. Graefe's new operation.

"We are justified in concluding that the percentage of corneal suppuration must be decidedly decreased by making the diameter of the flap, but particularly its height, as small as possible, and making the incision in the sclera instead of in the cornea; in short, by substituting the recently recommended 'modified linear extraction,' for the old flap extraction in sclerosed senile cataracts.

"The results obtained by this operation have thus far answered the expectations. Supported by a long series of cases, we may say that, in modified linear extraction, phthisis corneæ is very much rarer than in flap extractions; also that there is less fear of injurious reaction in the vascular parts, and that the capsule becomes more accessible, and consequently the detained portions of cataract may be more readily removed. Hence we may have less hesitation about operating on cataracts of normally consistent cortices or those which are adherent to the capsule, and in which the flap operation is very dangerous. On account of these advantages we readily overlook the disturbance of vision caused by the iridectomy, and the other objections to it. It would be too much to hope, by this operation, entirely to escape phthisis corneæ, and to certainly avoid fatal inflammation of the interior of the eye. The proceeding does not guarantee success, nor will it do so even with further improvements of the method of operating."

Wells' advice on this as well as other subjects is clear and decisive, and leaves no doubt on our minds as to his preference. Of v. Graefe's operation he says:—

"The success of this operation has been so great, that most ophthalmologists, amongst whom I may mention Mr. Bowman, have entirely abandoned the scoop extraction, and even to a great extent the flap operation. My own experience of it is also extremely favourable, and I prefer it greatly to every other mode of extraction."

He acknowledges that Dr. Taylor, of Nottingham, has operated by a somewhat similar method to that of v. Graefe, but quite independently of him, since 1865. The following summary of Wells is worthy of quotation:—

"I will now briefly mention the principal arguments which may be advanced in favour of or against the different operations for senile cataract. In doing this I shall confine myself to the flap extraction, the scoop operation, and von Graefe's new modified linear extraction.

"There cannot be any doubt that the common flap extraction is the most perfect operation of all, when it turns out perfectly successful. It

is nearly free from pain; it does not in the least interfere with the appearance of the eye; the pupil remains central and movable; the sight is perfect, and is not at all deteriorated and confused by circles of diffusion upon the retina, which are always more or less present when an iridectomy has been performed. It must, however, be confessed that these great advantages are often more than counterbalanced by the considerable dangers which beset the operation. On account of the great size of the flap, there is much risk of the vitality of the cornea becoming impaired, and of its undergoing partial or even diffuse suppuration, which may be accompanied by suppurative iritis or irido-choroiditis. Again, prolapse of the iris is a not unfrequent complication, proving a source of not only great annoyance and irritation, but even of danger to the eye. The after-treatment also demands much care and attention—more, indeed, than can generally be bestowed in an hospital, especially in a general one, with no special nurses or ophthalmic wards. Now, in the scoop extraction, these two principal dangers—suppuration of the cornea and prolapse of the iris—are nearly completely eliminated. On account of the position and shape of the incision, suppuration of the cornea, even of limited extent, is rare, and a prolapse of the iris can only be slight, and is confined to the angles of the section. Moreover, chloroform may be administered without any fear. But it must be admitted that iritis, chronic and insiduous irido-choroiditis, inflammation of the intra-capsular cells, and secondary cataract, are more common than in flap extraction. Von Graefe's operation, however, offers all the advantages of the scroop extraction, viz., the administration of chloroform, the linear shape of the incision, involving but a small portion of the cornea and the iridectomy, and yet one more most important one, the power of removing the lens without any traction instrument. It is in my opinion to be preferred, as a rule, to any other mode of extraction, more especially in hospital practice, as the patient requires far less watching and attendance, and the after-treatment is extremely simple. The confinement to the bed and house is also much shorter than in flap extraction. I think it is especially indicated in very feeble, decrepid, nervous, and unmanageable patients, or those suffering from severe cough or bronchitis; also if the pupil is adherent, or small and rigid, so that it dilates but imperfectly under the influence of atropine, or if the cataract is complicated with some choroidal or retinal lesion. It is also the safest operation for diabetic cataract, for in the flap extraction (even with a preliminary iridectomy), there is always some risk of suppuration of the cornea in these patients, as they are generally in a very feeble state of health. As the iris is exceptionally impatient of irritation and bruising in cases of diabetes, it may be advisable, in order to secure the greatest immunity from this danger, to make a double iridectomy, viz., upwards and downwards, so as to get a

broad vertical pupil, the two opposite portions of the iris being thus completely cut off from each other. I am sometimes asked by medical practitioners and students which operation I consider the easiest and safest for an inexperienced operator. I think that, all things considered, the downward flap operation is the easiest, for when the section has been successfully completed, the chief danger and difficulty are past; whereas in the modified linear extraction the iridectomy is superadded. I should, therefore, recommend that when the surgeon has operated several times by the lower flap extraction, and has acquired some experience and dexterity, he should pass on to the upper flap extraction and von Graefe's operation. The only two points in the latter which demand practice, care, and dexterity, are the incision and the removal of the lens. If the section is too small, the delivery of the lens will be difficult and forced, and will necessitate enlargement of the incision, considerable pressure upon the eyeball, or the introduction of some form of traction instrument. If, on the other hand, it is too large, and lies too far in the sclerotic, there is imminent risk of losing much vitreous humour, perhaps even before the removal of the lens is attempted. Considerable nicety and care are also required in coaxing out the lens by pressing upon the cornea with the curette, for if this is roughly and clumsily done the hyaloid may be ruptured, the vitreous escape, and the lens will probably be pushed somewhat aside, and a scoop will have to be employed for its removal."

Macnamara performs the following modified linear extraction without iridectomy:—When the pupil has been dilated by atropine and the patient chloroformed, he applies a spring speculum and steadying the eye with a fixation forceps he makes with a Jaeger's large triangular knife a straight incision nearly half an inch in extent into the cornea on its temporal side. As the blade of the knife is being withdrawn its point is run into the lens so as to rupture its capsule. A fenestrated scoop resembling Bowman's in all but its perforation, is then

"To be inserted so far into the anterior chamber as to enable us to reach the margin of the pupil; the handle of the instrument being raised and its rounded extremity depressed, the latter evidently rests on the capsule of the lens, immediately within the margin of the pupil. The scoop is now to be slightly withdrawn, still keeping its extremity on the lens, but so as to draw open the pupil far enough to enable us to press on the edge of the lens with the rounded extremity of the scoop; the instant this is done, the lens tilts over on its axis, and the scoop being thrust onwards, the lens comes to lie in its concavity, and may be removed from the eye."

By this means he frequently removes the lens in its capsule; he has operated on upwards of 600 cases by this method, and the results have surpassed those obtained by any other method; it is also, he says, applicable to either hard or soft cataracts, and is seldom followed by suppuration of the cornea even among the poor rice-fed Hindoos, in whom about thirty per cent. of eyes were lost from that cause after flap extraction. Professor Macnamara's remarks on iridectomy in cataract operations are so pertinent and important that we cannot close this portion of our subject without quoting them.

"I must here revert to the subject of iridectomy, and endeavour to determine how far its employment is necessary or otherwise in extraction, because if we satisfy ourselves that excision of the iris is not necessary, the range of our selection, as regards the method of operating, will be considerably curtailed.

"I may, I think, affirm that most surgeons will agree with Professor von Graefe, when he states that patients after iridectomy 'suffer certain disadvantages in vision in comparison with persons with a central pupil. These disadvantages do not apply so much to the acuteness of vision as they do to the dazzling caused by light.'[a] But it may be argued, if iridectomy decreases the danger of extraction, is it not better to run the risk of having an imperfect eye, rather than no sight at all? This would no doubt be sound reasoning, if we had not a very good chance of curing our patient without an iridectomy: but let us again refer to von Graefe on this point. In January, 1863, he states in the lecture above quoted from:—'I calculate that out of every hundred extractions, sixty-five yield a complete result. Of the remaining thirty-five fifteen yield perfect results from after-operations;' so that eighty per cent. of von Graefe's patients are cured by means of the flap extraction; nor is this all, for he goes on to say:—'Much more favourable statistics might be adduced under particular circumstances, *e.g.*, my own would be more than twice as favourable if I only included those who were operated on in the private apartments of my Institution.'[b] Evidently, therefore, all von Graefe's private patients must have recovered after the old flap extraction, and no less than eighty per cent. of his pauper patients gained admirable vision, its acuteness being one-fourth, or in persons beyond sixty-five, one-sixth.

"Moreover, Professor von Graefe says that 'iridectomy affords no protection against general or partial suppuration, but disposes the latter to a more favourable termination; it, moreover, prevents, up to a certain point, primary iritis and prolapse of the iris.'[c] Considering, therefore,

[a] Ophthalmic Review, Vol. i., p. 252. [b] Ibid., p. 248. [c] Ibid., p. 251.

that iridectomy has only an influence for good of a negative kind, but has a positively detrimental effect upon the perfection of vision, though not on its acuteness; and weighing these circumstances in the balance against the admirable results above quoted in favour of the old flap extraction, I must say my own inclination and experience strongly tend towards the operation, which enables me to remove the lens without excising any portion of the iris.

"I can hardly understand, after the Professor has thus pointed out the imperfection of vision due to iridectomy, and the doubtful way in which he speaks of the advantages of this proceeding in combination with extraction in 1863, why he should, in 1866, declare that this new operation of linear extraction, of which iridectomy is a prominent feature, 'may be accepted and practised to the entire exclusion of the flap method of operation.' In favour of this axiom, he states that ninety per cent. of his patients, operated on by the new method, gained an acuity of vision between one-sixth and five-eighths. Surely these statistics are not so greatly in favour of the new operation, as compared with those advanced when writing of the flap extraction; and let it be remembered that these ninety per cent. must all have suffered more or less from the 'dazzling' he mentions as a sequence of iridectomy.[a]

"My reasons for urging this point are by no means a desire to controvert or gainsay the opinions of so eminent a surgeon as Professor von Graefe, but simply because it appears to me we should aim at perfection in operating, unless it can be shown that our standard is incompatible with safety; and accordingly our ambition should be simply to extract, leaving a central and contractile pupil, rather than to remove the lens, plus a fourth of the iris.

"Iridectomy is, I conceive, most useful *after* extraction, if secondary iritis sets in, and the pupil will not dilate under the influence of atropine. And *at the time* of the operation, if there has been much difficulty or force required in removing the lens, or if prolapse of the iris, which we cannot reduce, occurs during the operation; and lastly, if adhesions exist between the capsule and the iris.

"Under other circumstances I am in the habit of performing either the old flap extraction, or the third form of linear extraction which I have described, without excising a portion of the iris. The latter, I am convinced, is a most valuable operation for the cure of all forms of cataract affecting the inhabitants of India, whether Europeans or Natives: perhaps, under different circumstances, it might not answer so well, but my success by this method has been more complete than by any other, and I cannot, therefore, but strongly advise others to give it a trial. Nevertheless, I am bound to state that surgeons of world-wide fame,

[a] Ophthalmic Hospital Reports, Vol. v., p. 357.

among whom I may mention Messrs. Bowman, Critchett, von Graefe, and Wecker, are evidently inclining, year by year, more strongly to iridectomy as a means of insuring the successful removal of the lens; and the opinions formed by surgeons of such eminence will doubtless have due weight in the profesion, leading them to set a higher value on iridectomy in extraction than I am at present disposed to assign it."

There is a decided difference of opinion regarding the advisability of chloroform in eye operations; we ourselves incline to Macnamara's views when he says:—

"Before closing these remarks on chloroform, I am bound to observe, that some of our most able surgeons, as for instance von Graefe, do not approve of chloroform. The latter only gives it to 7 per cent. of his patients, and now less frequently than he used to do. It may be that von Graefe is so skilful, and has had such vast experience, that he does not stand in need of the aid which chloroform certainly affords; but to the greater number of medical officers in this and other parts of the world, who will never have the opportunity of attaining such perfection, I can say with confidence, operate when your patient is fully under the influence of chloroform, and then with ordinary care and knowledge, your success will be very great; abstain from giving chloroform, and you may spoil innumerable eyes before you can attain the like happy results."

Chloroform saves suffering and anxiety, and the patient being unconscious and passive the most delicate operation is rendered comparatively easy. The beneficial effects and indeed the necessity of chloroform is especially witnessed in the operation of iridectomy for the relief of glaucoma. When we consider the great danger there exists of wounding the lens in this operation and producing cataract, the necessity of preventing struggling and having the eye perfectly quiescent, will be at once apparent. Moreover, we regard it as cruel to subject our patients to unnecessary pain.

The original edition of Stellwag von Carion is already so well known and favourably received, and its scientific character so well established, that the translation needs but little more at our hands than to bring it under our readers' notice.

The character of Professor Macnamara's Manual may be inferred from the quotations already referred to; it contains an epitomized and accurate account of the various diseases and affections of the eye and its appendages for which we may be consulted; the author's meaning and views are, though briefly, yet always well and clearly expressed. Personally we cannot but admire the indefatigable zeal and indomitable energy which can prompt

any one, circumstanced as the author of this work is, to undertake or even attempt any literary work. Engaged by the duties of a public hospital and of a professorship with its attendant teaching, and actively occupied by a large private practice in Calcutta, Professer Macnamara yet finds time and strength to devote to literature; and that he has acquitted himself with very marked success his present manual as well as his *Lectures on Diseases of the Eye*, 1866, and his *Treatise on Asiatic Cholera*, 1870, abundantly prove. When we consider his almost isolated position and his being debarred from witnessing the practice of, or personally exchanging ideas with, the leading oculists of Europe, his engagements and the temperature and climate of Calcutta, it is not surprising that his achievements should excite our wonder and admiration.

The manual is illustrated by 52 woodcuts copied principally from Stellwag's work, and by 12 coloured engravings from the magnificent plates of Dalrymple and Sichel; eight plates contain 26 chromo-lithographs of the *fundus oculi*. With a few exceptions these have already appeared in his *Lectures*, and are mostly representations of the fundus of the native Indian, some are copied from Jaeger, Liebreich and Galezowski; the sources of his illustrations being duly acknowledged. Being ourselves unacquainted with the ophthalmoscopic appearances of the Indian eye, we are incapable of pronouncing an opinion on most of the representations, but so far as the chromo-lithographs of the European fundus are concerned they are bad and inartistic. In the native Indian the fundus presents a greyish-brown appearance.

"This arises from the deep brown or black pigment which fills the hexagonal cells of the elastic lamina in the native, and entirely obscures the choroid, so that the fundus of the eye appears of a *brownish-grey* colour; whereas in the European, the incident light being reflected principally from the vascular network of the choroid, it is of a *crimson-orange* hue.

"The precise tint must of course vary in each individual, depending upon the depth of colour of the pigment contained in the hexagonal and choroidal cells; for instance, the inhabitants of Bengal being comparatively fair, the fundus of the eye, when examined with the ophthalmoscope, appears of a grey colour, whereas in the very dark-skinned native of the South of India it will be almost black." [a]

[a] Dr. Smith, Prof. of Ophthalmic Medicine, Madras Medical College; *Madras Quarterly Journal of Medical Science*, No. 15, Feb. 1864.

The colour of the optic papilla is "of a more decided pink hue in the native than in the European eye, where in fact it is of a whitish-yellow colour; but it may be of a lighter or darker pink, or yellow without there being any reason to suppose the part is diseased." The chapter on the examination of the eye contains special information on the principles and use of the ophthalmoscope; the author prefers for his own use, as does also Wells, Coccius' ophthalmoscope, but observes that where a Liebreich has been habitually employed it is not desirable to change it for a Coccius. In describing Liebreich's hand ophthalmoscope the author's nomenclature is we think if not incorrect at all events open to criticism; he says "attached to the mirror is a clamp by which one of the *object glasses*, to be found in the instrument case, may be fixed *in front* of the sight hole;" (the italics are our own). In the now generally accepted nomenclature the object glass signifies the large convex lens held in front of the examined eye by means of which the image is formed between it and the examiner; and the clamp or clip holds the small *ocular* lens *behind* the sight hole, the reflecting surface of the ophthalmoscopic mirror being regarded as its front. The author prefers sunlight for ophthalmoscopic examination, and recommends the interposition of a smoked glass in front of the mirror or of a blue glass in front of the patient's eye, so as to moderate the glare.

"The advantages of sunlight are so great, that it should always be used if practicable; the colour of the fundus of the eye is more accurately seen, and the character of abnormal changes determined with greater precision, than if artificial light be employed.

"Should sunlight be preferred, the most convenient plan to adopt is to send an assistant out into the sun with a mirror, from which the rays of light can be reflected through an open door or window into the room. The patient should be seated with his back to the window, and the observer opposite to it, so that the reflected light from the mirror may be thrown upon his face. He then closes one eye, and applying the ophthalmoscope to the other, the rays of light are received upon its polished surface, and directed into the patient's eye."

Scirrhus is the most common form of cancer met with in the orbit in Calcutta, and the author coincides with Tyrrell in deprecating operative interference.

"With regard to the treatment of scirrhus of the orbit, I am opposed to any surgical interference in the first stages of the disease; the use of

the knife, I believe, accelerates its progress, and the chances are infinitely small of our being able to eradicate it. I have, in the early stages of scirrhus of the orbit, removed the whole of the structures contained within it, leaving its walls entirely bare, so as to present an appearance very like a dried specimen, and yet the disease has speedily reappeared and run a very rapid course. I think it better, therefore, to leave the patient alone; the alternative is a fearful one, and it is frequently most difficult to abstain from an attempt to do something for the relief of the sufferer."

He has tried Dr. Broadbent's plan of injecting acetic acid, and says that although it has failed in his hands, it may be well worth a further trial.

Mr. Lawrence has lately brought the operation of removal of the lachrymal gland again under the notice of the profession, and some discussion has arisen as to its effects upon the moisture of the conjunctiva. In explanation of the now established fact that the conjunctiva does remain moist after even the complete removal of the gland, Professor Macnamara says:—

"It will be well to bear in mind, in dealing with cases of lachrymal disease, that the surface of the conjunctiva depends principally for its moisture upon the secretion from the conglomerate and conjunctival glands, and not upon the lachrymal gland. Hence, in cases where the secretion of the gland is abnormally great (epiphora), causing a constant flow of tears over the cheeks, other means of treatment having failed, we may safely resort to excision of the gland, without fearing that the natural moisture of the eye will be interfered with. Thus, also, we scarcely ever find dryness of the eye a subject of complaint with patients suffering from loss of the lachrymal gland, whether arising from chronic inflammation or any other cause."

And in explanation of the blindness and ptosis which supervene occasionally upon injuries and wounds of the lids and supra-orbital nerve, he says:—

"In order to explain this connexion, we must suppose that some of the branches of the fifth nerve have been injured, that the irritation has extended thence to the ophthalmic ganglion and carotid plexus, and ultimately involved the sympathetic in the morbid action: that, in consequence, the capillaries of the retina and vessels supplying the peripheral branches of the third or motor nerve become first contracted, and subsequently dilated from exhaustion, a permanently engorged state of the capillaries being induced, which interferes with the nutrition of the nervous elements; these become atrophied, and the result is the condition

above noticed. However this may be, it is well to bear in mind the fact, that even apparently trivial injuries to the eyelids are sometimes followed by paralysis of the levator palpebræ, or by total loss of sight in the injured eye."

Professor Macnamara condemns the use of leeches and calomel, and says that he has not found it necessary to use a leech or give a grain of calomel to his patients in the Calcutta Ophthalmic Hospital during the last year, although he has a daily average of fifty in-door patients under his care. In his capacity of teacher, however, he feels himself "bound to a certain extent to set forth the views of some of the leading surgeons in Europe" as well as his own, and therefore mentions mercury occasionally as part of the treatment which may be adopted. In the chapter on iritis, he says:—

"As a general rule, it may be affirmed that mercury is never to be employed in iritis, unless in cases arising directly from inherited or primary syphilis; in other cases this drug is useless, and in very many positively injurious. In instances, however, of inflammation of the iris, apparently originating in an injury to the part, if the patient be labouring under a syphilitic diathesis, the effects of the mercurial vapour-bath may be tried; in complicated cases of this description its beneficial effects are said to be very marked. I would repeat, that it is seldom necessary to give the drug by the mouth, unless in rapidly advancing cases, it being far more under control when used in the form of inunction or as a vapour-bath; and if the mercury does not appear to check the progress of the disease before salivation is induced, it is useless continuing its employment—that is, the treatment should never be pushed to such an extent that the harmful influence of the drug on the system becomes developed." [a]

We, ourselves, do not endorse his views respecting the inadvisability of giving mercury, being of opinion that it is in many cases our sheet anchor.

Professor Macnamara's work contains a vast quantity of information compressed into a small space, and is we consider the best manual on diseases of the eye now existing; although specially adapted for students, it will be found reliable and serviceable by the busy practitioner. Its marginal notes on the contents of the paragraphs will greatly facilitate reference.

Professor Soelberg Wells is already favourably known through his admirable work on *Long, Short, and Weak Sight*, and by his papers on Strabismus and other subjects; his present *Treatise on the*

[a] Langston Parker "On Syphilitic Diseases," 4th edit. p. 23.

Diseases of the Eye must enhance his reputation as an author and a practitioner. The work is well and fluently written, its diction and composition are pleasing and attractive, and the author's views and practice clearly and definitely expressed. It is characterized throughout by an intimate acquaintance with the subjects treated of, and by broad and liberal views. It is moreover written up to the moment of its publication, and contains information on the latest novelties, such as Stilling's "Cure for Strictures of the Lachrymal Passages by Internal Incision" (a method very similar to one described by Jaesche in the *Archiv für Ophthalmologie* for 1864), and Wegner's "Views on the Nature of Glaucoma," subjects amongst others which are absent from Stellwag's work. Another praiseworthy feature in the work is the fact of its pages being cut, and of its being well indexed and remarkably well printed on good paper, matters which are we think not sufficiently attended to by authors and publishers. Messrs. Churchill deserve every credit for their part in the publication of this really charming volume.

We have already given several quotations from this work, and we will now briefly touch upon a few more of the many practical and interesting subjects contained in it. When the eyelids are much swollen and tense, during purulent ophthalmia, and when they press greatly upon the eyeball, and the cornea is threatened, the author recommends division of the outer commissure of the lids. This incision is to be made through the skin and fibres of the orbicularis, but not through the mucous membrane.

We have witnessed this procedure carried out with marked success by v. Graefe; and it is a practice especially applicable to children as a substitute for leeching. It is easily and expeditiously performed and the bleeding easily controlled; by holding the wound open the bleeding is encouraged; by allowing the lips of the wound to close the bleeding generally ceases; if necessary a small compress can be applied. We have on many occasions advocated in these pages the more general employment of atropine—almost to the exclusion of nitrate of silver, which we regret to see still in such general use. Our three authors all agree in largely recommending the employment of atropine in ophthalmic practice. Macnamara thus refers to its action in iritis:—

"*Atropine* is invaluable in the treatment of all cases of iritis, for if we can only manage to keep the pupil dilated, it is impossible for synechia to form; besides which, when the iris is well contracted on itself, forming a narrow rim round the anterior chamber, it follows that its blood-vessels

cannot remain in a state of congestion; they must, in fact, be pretty well emptied of their contents. The inflamed tissue is also kept at perfect rest when under the influence of mydriatics—a most important point in the treatment of all forms of inflammation. I know, indeed, of no remedial agent which directly places an inflamed structure in a more favourable position for recovery than atropine does in cases of iritis; it enables us to secure the inflamed part absolute rest, it lessens the calibre of its congested vessels, and it prevents the injurious adhesions which otherwise follow an attack of plastic or parenchymatous iritis, by keeping the iris well away from the lens; and lastly, by lessening the vascular supply, and diminishing the amount of secreting surface of the iris, it controls the secretion of the aqueous humour, and in this way lessens the intra-ocular pressure."

When discussing its merits in corneal affections, Wells says:—

"It exerts a beneficial influence upon the cornea, by acting as a local anesthetic during its passage through the cornea into the aqueous humour, thus greatly diminishing the irritability of the cornea and of the ciliary nerves. This is often witnessed when a drop of atropine is applied to an eye affected with acute corneitis, accompanied by intense symptoms of irritation; for if such an eye is examined half an hour after the application of the atropine, we find a very marked diminution in all these symptoms; the patient expressing himself greatly relieved. The atropine also acts by decreasing the intra-ocular tension, and thus relieving the cornea of a certain degree of pressure; hence its nutrition and the regeneration of its substance are greatly facilitated. This diminution in the intra-ocular tension is of special advantage in deep ulcers of the cornea, as will be readily understood when we remember that the thinnest portion of the cornea (the bottom of the ulcer) has to sustain the same degree of intra-ocular pressure as the healthy part."[a]

[a] I must, however, strongly insist upon the absolute necessity of the solution of atropine being quite pure, and perfectly free from any admixture of strong acid or spirits of wine. A few drops of strong sulphuric acid are sometimes added by chemists when the sulphate of atropine is not quite neutral, and therefore imperfectly soluble. I have met with several instances in which a pure solution of atropine proved of the greatest benefit in allaying the irritability of the eye and in alleviating the inflammation, and in which a fresh supply of atropine (made up after the same prescription, but obtained from a different chemist) has at once set up severe irritation of the eye, accompanied by considerable pain, redness, and lachrymation, &c., but these symptoms soon disappeared again on the use of a *pure* solution of atropine. On examination, the impure solution was found to contain a small quantity of strong sulphuric acid. Such cases as this completely disprove the theory that a small quantity of strong acid or of alcohol can have no prejudicial effect upon the eye, even although there may be much ciliary irritation and a severe inflammation of the cornea or iris. I must state, however, that we occasionally meet with exceptional cases, in which there exists

We may mention that the Medical Council, on being made acquainted with the unsuitability for ophthalmic practice of their original solution of atropine, have introduced into the new edition of the *British Pharmacopœia* a very excellent one of sulphate of atropia—a preparation which has been employed for some years by Sir William Wilde. Sometimes atropine will not produce its effects in iritic inflammations until after depletion by cupping or leeching, and this circumstance, which we have often heard Sir William Wilde dwell upon at his clinique, has not escaped the accurate observation of Wells, who says:—

"But we sometimes find that the action of even a strong solution of atropine, frequently applied, is resisted, and that it produces little or no effect, and increases rather than diminishes the irritability of the eye. In such case its use must be desisted from until the irritation is relieved by the application of a few leeches to the temple, or perhaps by paracentesis of the anterior chamber. This relief of the inflammatory irritation and intra-ocular tension, permits of a freer absorption through the cornea, and hence the effect of atropine will now be often very marked and rapid. This effect, as von Graefe has pointed out, is sometimes noticed without the re-application of the remedy. This atropine may have been applied in cases of iritis or corneitis without producing any dilatation of the pupil, but many hours afterwards this has ensued after the application of leeches. We sometimes notice, also, that although dilatation of the pupil may have been produced, yet that it cannot be thoroughly maintained, the atropine appearing to lose its effect. In such cases it will be found that this likewise is due to the great irritation of the eye and the increase in the intra-ocular tension, which prevent the absorption of the remedy through the cornea. Whereas, after the application of leeches, or the performance of paracentesis the atropine will again regain its power over the iris."

In ulceration of the cornea we often find an accumulation of pus in the anterior chamber, without any apparent opening in the cornea through which the pus might gain access; and in the absence of

a peculiar idiosyncrasy which renders the patient most intolerant of the use of even a weak and perfectly pure solution of atropine. I have seen instances in which a drop of a weak and quite pure solution of atropine has produced great irritation and pain, or even an erysipelatous condition of the eyelids and cheek accompanied by redness and chemotic swelling of the conjunctiva. This is, however, a very exceptional occurrence, and bears not the least analogy to those cases in which the irritation is caused by the impurity of the atropine, for in such cases a pure solution of atropine is not only well borne, but greatly alleviates the ciliary irritation and the inflammatory symptoms. Mr. Lawson also mentions some interesting instances of this peculiar idiosyncrasy, in a paper in the "R. L. O. H. Reports," vi. 119.

suppurative iritis—a not uncommon complication—it is difficult to understand the source of the pus or lymph. The following explanation of Wells is therefore very interesting:—

"The abscess may perforate the cornea, and its purulent contents be carried into the aqueous humour and be precipitated at the bottom of the anterior chamber. Sometimes such a mode of production of hypopyon is completely overlooked, from the fact that the communication between the anterior chamber and the abscess in the cornea is not large and direct, but is brought about by a small sloping canal, through which the contents of the abscess have made their way into the anterior chamber. Special attention has been called to this fact by Weber, who has, moreover, frequently passed a minute probe from the ulcer through the canal into the anterior chamber, and thus verified the communication. With the oblique illumination, this little canal appears like a white streak, running from the abscess to the anterior chamber."

In threatened perforation of the cornea, and in cases of purulent collections in the anterior chamber, Wells recommends paracentesis of the cornea, as do also both Stellwag and Macnamara, not so much with the object of evacuating the pus as to lessen the intraocular pressure. When the accumulation occupies one-third or one-half of the anterior chamber, if there is much iritis, or if the abscess in the cornea is deep and threatens an extensive perforation, it is, Wells says, of great importance to perform an iridectomy without loss of time, for the intra-ocular pressure will be thus more completely diminished, and for a longer period than by merely opening the cornea; the progress of the disease will be arrested, the absorption of the morbid products hastened, and the regeneration of the corneal tissue facilitated. Nature herself we think points out to a certain extent this plan of treatment, for with the bursting of the cornea, which takes place in such a large majority of these cases, the pain ceases and the reparative process commences.

When treating of corneitis the author combats the doctrine of its syphilitic origin so ably and philosophically worked out by Hutchinson.

"I cannot at all agree with the view that diffuse corneitis is always due to inherited syphilis, for although I have often seen it associated with the latter, yet in many cases not the slightest trace of a syphilitic taint could be ascertained, and there was a marked and complete absence of the peculiar syphilitic features and the notched teeth. Indeed, I think that we are often too apt hastily to jump to the conclusion that hereditary syphilis exists, when on a more careful and searching

examination into some of these histories, it would be found that the miscarriages, early deaths of the children, &c., were due to perfectly natural causes, and quite independent of any syphilitic taint. I may of course be met with the constantly recurring argument that it is impossible to get at the truth of the history, but I think that we are justified in giving the patient and his parents the benefit of the doubt, if no reliable proof of the presence of inherited syphilis can be made out. For this reason, I must completely disagree with those authors who term this disease 'syphilitic corneitis.' For, as I have already stated, it is frequently met with in persons, in whom not the slightest trace of a syphilitic taint can be detected. Whilst combating some of these views, I must, however, seize this opportunity to express my admiration for the very important and interesting researches of Mr. Jonathan Hutchinson, into the frequent connexion between inherited syphilis and many of the diseases of the eye, a discovery which has proved of great importance and use in the treatment of these affections."

We perceive that Wells employs the term *corneitis;* we ourselves prefer the term *keratitis*, deeming it on philological grounds the more correct designation.

The great practical importance which attaches to the subject of sympathetic inflammation must be our excuse for again harping on this subject; we would desire to keep constantly before our eyes the insidious nature of this malady its lamentable results and the dangers of delay in taking prompt and active measures for its suppression or prevention. Wells adduces evidence to prove that the ciliary nerves are the channel through which the irritation is propagated from the originally affected eye to the sound one. The following very practical remarks on the prevention of the occurrence of this malady are deserving of our best attention:—

"As far as I am aware, no instance has been recorded in which sympathetic ophthalmia ever attacked an eye after the injured eye had been removed, if at the time the other was still quite unaffected. This being so, there cannot be the slightest doubt as to the imperative advisability of the immediate removal of an eye which has been so greatly injured as to have quite lost its sight, or at all events to leave no hope of any restoration of a useful degree of vision. This is still more the case if the injury has been of a kind which is prone to be followed by sympathetic ophthalmia. For we have no guarantee that we shall have time to check the sympathetic inflammation if it has once broken out, even by a speedy removal of the injured eye. For although symptoms of sympathetic irritation not unfrequently usher in the inflammation, and the latter may be prevented by the excision of the

injured eye at this promonitory stage, yet this is not always the case. The inflammation may occur without any premonitory symptoms, and advance so rapidly that in the course of a few days the integrity of the eye may be greatly and perhaps permanently impaired. Thus, a case is narrated by Maats, in which within four days (and without any premonitory symptoms) an eye became so affected by sympathetic irido-cyclitis, that there was nearly a complete posterior synchia, and the sight had sunk to $\frac{3}{200}$. In spite of the immediate removal of the injured eye, and of every endeavour to improve the condition of the other by iridectomy, and subsequently by a second iridectomy, with removal of the lens, the eye become atrophied, and only retained perception of light. Such a case should warn us of the danger of procrastination in excision of the blind injured eye, in the hope that there will always be time enough for this when symptoms of sympathetic irritation manifest themselves or during the earliest stage of sympathetic inflammation. For the former may never occur, and the latter may be so rapid in its development and course, that great and irremediable mischief may be done before we can enucleate the other eye. Moreover, there is another point which weighs heavily in the scale amongst persons whose livelihood depends upon their work, and that is the long time which is lost by them during the treatment of the injured eye. It may be laid down as a fundamental rule, that as long as the injured eye remains painful to the touch it is always a source of danger, and may at any moment set up sympathetic ophthalmia. It should consequently be removed if its sight is lost, or greatly and irremediably impaired, this being particularly indicated if a foreign body remains within the eye. For thus only can we insure the patient against the dangers of sympathetic inflammation. The question as to whether the injured eye should be removed if it still retains some degree of vision is of course much more difficult and embarrassing. In deciding upon this point, we must be chiefly guided by the nature and extent of the injury. Thus, if it is a small incised wound of the cornea or sclerotic, and the iris, lens, and vitreous humour have escaped any severe injury, we may by careful and judicious treatment avoid the danger of sympathetic inflammation, and ultimately, perhaps, restore excellent vision. But if the wound is very extensive, and implicates the ciliary region and sclerotic, if the lens has been lost or is injured, a considerable amount of vitreous humour has escaped, or intra-ocular hemorrhage has occurred, and if, consequently, the injuries are so great that but very little, if any, sight can possibly be saved, it is much better to remove the eye at once, even although some degree of vision may still exist. Still more imperative is such a course, if these extensive injuries are due to a foreign body which has become lodged in the eye and cannot be removed by operation, for although rare instances occur in which foreign bodies remain encapsuled and quiescent within

the eye, such cases form, unfortunately, the great exception. I would especially urge the necessity for the operation, if the patient resides at a distance from medical aid, so that a careful watch cannot be kept over the eye, and the first symptoms of sympathetic irritation or inflammation be at once detected. The question in all such cases, whether it is not better to sustain a small loss than to run the risk of a very great danger. I, however, fully feel and admit the heavy responsibility which rests upon the surgeon who shall advise the removal of an eye which still possesses some sight, and when, as yet, no symptoms of sympathetic disease have appeared. We can in such cases only carefully and conscientiously weigh the different bearings of the case, and place them clearly and forcibly before the patients and his friends, and leave the decision in their hands. I have entered somewhat at length upon this part of the subject, because I feel it to be of great importance to all medical men, and one upon which they should hold strong and decided views. For we never know at what moment we may not be called upon to decide a question of this kind, and what reproaches we may not have to make ourselves if by our procrastination and indecision the second eye is lost from sympathetic ophthalmia."

The theory and use of the ophthalmoscope are well and fully treated of in Chapter 6; and the diseases of the vitreous, retina, choroid, and optic nerve done full justice to. In retinal detachment with a collection of fluid between the retina and choroid he advocates Mr. Bowman's operation of introducing two needles through the sclerotic, choroid, and retina, into the vitreous humor; the object is not so much to evacuate the subretinal fluid externally as to allow of its percolation into the vitreous chamber, and thus prevent its further extension between the retina and choroid.

At p. 441 will be found the record of a most interesting case of tubercles in the choroid which the author diagnosed during life and in which he demonstrated the tubercles after death. In 18 cases of miliary tuberculosis, which underwent *post mortem* examination, Cohnheim found tubercles in the choroid of one or both eyes *in every instance.*

There are very many other subjects in this work which our limited space forbids our alluding to. His chapter on Glaucoma is especially interesting, and contains an ample discussion on its nature and treatment. He, as well as Stellwag and Macnamara, emphatically recommend iridectomy, and he says of this operation:

"Some writers have stated that the operation of irridectomy, as it is to be performed in glaucoma, is just the same as the old operation for

artificial pupil. Nothing could be more erroneous. The principle of the two operations is perfectly different. In the old operation, an opening was made in the cornea, and a small portion of iris, in proportion to the desired size of the pupil, excised. In the modern operation of iridectomy for glaucoma, the chief point is to make the incision in sclerotic, or at the sclero-corneal junction, and of a sufficient extent to permit the removal of a large segment of the iris (about one-fifth) quite up to its ciliary attachment. The more intense the symptoms, the more considerable the increase in the intra-ocular tension, the larger should the iridectomy be. Many of the negative, or only partially successful, results which have followed the employment of iridectomy in glaucoma, were undoubtedly often due to some fault in the performance of the operation. Either too small a portion of the iris was excised, or it was not removed quite up to its ciliary attachment. We sometimes find that if only a small portion is removed, and this is not up to the ciliary insertion, the symptoms do not completely yield, and more or less increase of tension remains. If, in such a case, a second and larger iridectomy is made, and the iris removed quite up to its ciliary attachment, the beneficial effects at once become apparent, the tension diminishes, the inflammation subsides, and the vision improves."

The work is illustrated by 104 wood engravings and 6 plates containing 16 chromo-lithographs illustrative of the diseases of the fundus. These are *fac-similes* of some of Liebreich's magnificent figures, and are executed by the same artists. They are though beautiful not quite equal to the originals in colour and tone, owing we believe, to the circumstance that Liebreich, who is not only a good artist but also a skilled workman, himself superintended and assisted in the mechanical production of his atlas.

The perusal of Professor Wells' work has given us very great pleasure, and we confidently recommend the book to the profession, knowing it to be a most trustworthy and satisfactory practical guide and counsellor.

Dr. Wolfe's brochure is chiefly a reprint of the author's papers in the *Lancet*, and as these papers have been written assignedly with the view of bringing before the profession an improved operation for the removal of cataract, we shall at once proceed to the consideration of this operation.

"The author's method," briefly stated, consists in flap extraction, with a small corneal section, and Desmarres' well-known "bridge," performed six weeks after an iridectomy, upwards or downwards We are at a loss to perceive any great novelty in this operation.

Mooren, whose operation the author describes at p. 44, introduced the performance of iridectomy some weeks previous to a flap extraction. Desmarres, under whom Dr. Wolfe studied, as is evident not merely from the dedication page but also from the style and general tenor of the work, introduced the "bridge;" this feature therefore as well as the previously performed iridectomy are not new, and the only remaining improvement is the smallness of the corneal flap; this flap "extends to a line more than one-third of its circumference;" the rule laid down for the old flap extraction was that one-half of the corneal circumference should be opened, but in practice the section not unfrequently falls out unavoidably less; there is not a very great difference between one-half and "a line more than than one-third," and we maintain that this fine and accurate gauging of measurement is only possible when chloroform is administered and the eye steadied by fixation apparatus, and as Dr. Wolfe gives chloroform only in exceptional cases, and employs Pamand's ophthalmostat, which is acknowledgedly less serviceable for fixation than forceps, we conceive the same difficulty as to the extent of section must arise in this as well as in the ordinary flap extraction. In expressing the lens after laceration of the capsule, Dr. Wolfe uses his fingers only, and as other operators have done likewise years ago, we see nothing novel in it, although figured as the "author's method."

This "improved method of extraction of cataract" occupies $11\frac{1}{2}$ pages of type, a portion of which is taken up by tables, and the notes of cases, including some cases successfully operated on in private by the old flap extraction!

The tabular statements are very meagre, the object of the first would seem to be to show that of the 107 operations, 35 were in private practice; the other table shows the results but contains no allusion to the comparative results in private and in hospital.

The loss in these 107 cases by internal hemorrhage, suppuration, and iritis, was only 4—a most satisfactory result. Under the heading "healed well, but no vision," we find 7 cases, 2 of which were glaucomatous, but like the patients themselves we are left in darkness respecting the cause of blindness or why they were operated on; 94 "healed well, with perfect vision," and of these one was a glaucomatous cataract, so we presume the iridectomy cured the glaucoma, as the patient recovered *perfect* vision; only 2 required subsequent operation. We may mention that Mooren who obtained by his flap extraction with previously performed iridectomy the

very small percentage of 7 per cent. loss has abandoned his operation in favour of v. Graefe's new one.

The padding of Dr. Wolfe's book is composed of a meagre history of extraction, of remarks on the removal of cataract by absorption, couching, Schuft's, Bowman and Critchett's, Mooren's, Jacobson's, and v. Graefe's operations, and of Churchill's *Catalogue* for October, 1868; some pages are also devoted to glaucoma and the operation of performing iridectomy. As his manner of performing iridectomy "differs in some respects from the methods usually adopted," he figures and describes his method, but we have failed in recognizing in what the differences consist. In the section on v. Graefe's operation are figured a cystotome and a hook which Dr. Wolfe designates as his ("my.") If by "my" we are to understand anything more than mere ownership we are at a loss to understand in what the originality consists, so far at least as the hook is concerned. The cystotome which Dr. Wolfe claims as his, has "a blunt convex surface and a sharp edge at its convexity," two opposite conditions which we find a difficulty in reconciling.

The work is illustrated by several plates, in one of these we find a cut of "the author's cataract knife," but fail to see any novelty in it. The illustration of the "author's method—first period of extraction," has we confess caused us no little surprise; the knife is represented as held by the fingers, in the same manner as a pen is held in the act of writing! If this be a true representation we are filled with wonder at the author's ingenuity in devising something astonishing, and going so far out of his way to create a difficulty. We would refer our readers to Mackenzie for a most excellent and truthful illustration of the mode of holding the extraction knife. But we cannot for a moment believe that Dr. Wolfe or any one else would attempt extraction in the manner figured in his book. The work appears to be very carelessly revised, as the following amongst others will show. "With the right hand I fix the globe by means of the pique de Panand, which I consider the most suitable instrument; with the narrow Beer's knife in my right I enter, &c."

Mr. Carter's English edition of Scheffler's treatise reads more like an original work than a translation, and bears ample evidence to the translator's practical familiarity with the subject as well as to his knowledge of German and his powers of expression in his

own language. Three quarters of the work is a translation of the German edition; the remainder consists of new matter furnished by Dr. Scheffler for the English edition. The treatise appears to be a continuation of two previously published works by the same author, viz: *Die Physiologische Optik* and *Die Gesetze des räumlichen Sehens;* and it is almost absolutely necessary for the proper comprehension of the work now before us that the reader should be acquainted with these works or else possess a considerable knowledge of mathematics. In order to render the work as independent as practicable of the two books alluded to and comprehensible to the English reader some prefatory pages explanatory of the author's optical formulæ and symbols have been added by the Rev. W. de Lancey Lawson.

One of the principal objects of the work is to show that in the choice of spectacles we should take into consideration not only the refractive condition and optical adjustment of the eye, but also the state of convergence of the eyes towards each other, and the state of another function which the author appears to have established and to which he gives the name *Application.* The author considers that orthoscopic spectacles (which consist of a prism added to the spectacle lens) are "the most useful for large classes of ocular defects; and that they rest upon the only rational basis of spectacle construction for binocular vision." Such spectacles he says never produce *strain*, nor do they augment such strain as may already exist in the unaided eye.

The author views as erroneous the present doctrine which teaches that when an eye fixes upon an object it looks at this object in the direction of its visual line, and adduces what would appear to be conclusive evidence that it sees the object in the direction of the frontal axis (a line drawn forwards from and perpendicular to the root of nose). The two conditions for single vision are he says that the apparent directions for the two eyes should have no deviation from each other, that they should therefore be parallel, and that both apparent directions should have a common point of origin—the frontal axis. He establishes two lines of direction, the apparent from the frontal axis to the object and the actual from the eye to the object. According to laws laid down by the author in a previous work the apparent direction undergoes a deviation from the actual in which the angle is, in a normal pair of eyes, equal to half the angle of convergence of the ocular axes towards each other. This deviation is not dependent

upon the participation of the other eye in the visual act; it occurs even in a greater degree when the other eye is excluded. The author considers this deviation to be a sensational process analogous to accommodation and to fixation, and to this faculty which is subservient to the knowledge of the direction of objects he gives the special name of *Application.* This Application the author says: "Since it affords a definite sensational feeling, must rest upon a definite nervous process, and must therefore have a definite material basis. I believe that the *mobility of the bacillary layer of the retina*, of which, from the deductions in my *Physiologische Optik* and *Gesetze*, there can no longer be any doubt, forms the basis of the Application." The direct motor agent which produces this mobility is not explained by the author. There are two forms of mobility, viz: shifting of the bacilli in the retina, and secondly, change of inclination of the bacilli with reference to the perpendicular of the retina. The application or recognition of apparent direction depends on this latter form of mobility which is termed nutation of the bacilli. This forms one of the most attractive and interesting, though possibly erroneous theories which has come to our knowledge; it is admirably worked out, and Schultze's wonderful investigations on the retina are adduced in its support. Without diagrams and more space than is at our disposal it would not be possible to give a satisfactory explanation of the author's views, and we can only refer our readers to the work itself.

The author describes two principal groups of defects of application, one depending upon an abnormal or non-radial position of the bacilli upon the retina in a state of rest—anomalies of position; the other depending upon inervation or inability of applying accurately—anomalies of application or of nutation. These are again classified into several subdivisions. He considers that prisms will correct or counteract the faulty bacillary motions.

Based on these views the author promulgates a new theory respecting the cause of squint. Donders attributes the divergent strabismus of the hypometropic eye to muscular insufficiency and the convergent strabismus of the hypermetropic eye to abnormal effort of accommodation. Scheffler, holding that according to this doctrine double vision must ensue, regards strabismus as depending upon a close interdependence of the accommodation and the application, and explains that in hypometropia and in hypermetropia the nutation necessary to render the bacilli parallel to the frontal axis is too great, and that in consequence of the small

nutation double vision with crossed images would ensue were it not for the craving after single vision which compels the eye to roll outwards or inwards respectively according to the variety of the anomaly. Based also on this theory of the mobility of the bacilli he gives a novel and plausible explanation of the choroidal atrophy in myopia. This atrophy as well as the morbid changes in the retina and optic papilla he regards not as the cause but as the consequence of the myopic refraction. The external extremities of the bacilli dip into the pigment of the choroid; in myopia there is a great and constant nutation of the bacilli outwards which produces a corresponding and unnatural stretching of the choroid and retina at the pole of the eyeball; hence the myopic crescent, altered diameter of the papilla, &c. We would commend the work to all thoughtful workers, its value is greatly enhanced by the chapter on Schultze's investigations into the microscopical anatomy and physiology of the bacillary layer of the retina; these researches and conclusions are some of the most important in modern times, and we feel indebted to Mr. Carter for having brought them as well as Scheffler's opinions so ably under the notice of the English profession.

It is not long since we introduced our readers to Mr. Lawson, by means of his most valuable and eminently practical work on *Injuries of the Eye*. The same author now comes before the public with a small manual, which is like all the author's writings—truthful and practical; it is also a good mirror of the practice at the Moorfields Ophthalmic Hospital, an institution which is justly celebrated for its progressive tendencies, and its numerous and brilliant staff of teachers. Mr. Lawson's *Diseases and Injuries of the Eye*, now before us, is a concise and useful little manual, exceedingly well brought out by the publisher, and one we can specially commend to students.

It may be recollected that in the first portion of this notice, in our February number for 1869, we commented upon the fact of ophthalmology being almost altogether ignored by the Universities and licensing bodies in this country, where eye diseases are so prevalent. It affords us much gratification to find that since then the University of Dublin has, with that spirit of liberality and enlightenment which has actuated her governing body for many years past, taken the initiative in setting aside this reproach. According to a notice of the senior lecturer's, dated 6th August, 1869, candidates

for the degree of Master in Surgery must in future produce certificates of practical instruction in ophthalmic surgery. We hope this may be but the first step towards other necessary changes.

WORKS ON DISEASES OF WOMEN.

1. *The Practice of Medicine and Surgery applied to the Diseases and Accidents incident to Women.* By Wm. H. Byford, A.M., M.D.

2 *Obstetric Clinic: a Practical Contribution to the Study of Obstetrics, and the Diseases of Women and Children.* By Geo. T. Elliot, Jun., A.M., M.D.

3. *The Diagnosis, Pathology, and Treatment of Diseases of Women, including the Diagnosis of Pregnancy.* By Graily Hewitt, M.D., Lond.; F.R.C.P., &c. Second edition, revised and enlarged, with numerous illustrations.

4. *A Handbook of Uterine Therapeutics, and of Diseases of Women.* By Ed. J. Tilt, M.D., &c. Third edition.

5. *Pathological Anatomy of the Sexual Organs.* By Julius Klob, M.D., Professor at the University of Vienna. Translated from the German by Joseph Kammerer, M.D., and Benjamin F. Dawson, M.D.

We have before us new editions of four very important works on the diseases peculiar to women. They are already well known and justly esteemed, so that there remains for us but to announce their appearance, and draw the attention of those of our readers who have not hitherto made their acquaintance to some of their leading peculiarities.

Dr. Byford's is the second edition of a systematic treatise on the diseases of women, including puerperal convulsions and puerperal fever. While it gives a good summary of what is known on these subjects, well digested, and clearly and plainly expressed, it is remarkable beyond most systematic treatises for the care and fulness with which the sympathetic symptoms of uterine diseases are described. Chapter X., which treats of chronic inflammation of the uterus and cervix, contains one of the best descriptions of these symptoms that we remember to have met with, and is evidently the production of a careful and experienced investigator, and one fully

endowed with "a medical mind." If our space permitted we should gladly extract the whole of this chapter, for we believe with the author that those symptoms are often considered independent affections, and their true origin is either not suspected, or overlooked, and if they were not misunderstood, an immense amount of suffering that is now borne as a matter of necessity might be relieved. Dr. Byford describes in succession sympathetic affections of the stomach, such as anorexia, voracity, depraved appetite, and gastralgia; sympathetic affections of the bowels, such as constipation and diarrhea, and specially alludes to the affection in which muco-fibrinous casts of the intestine are discharged, a condition described by some of the older writers, but greatly overlooked till Sir James Simpson drew attention to it, under the name "pellicular enteritis;" it is, however, attributed by Sir James to an eruptive disease of the intestinal mucous membrane rather than to uterine disease. Sympathetic affections of the liver and of the nervous system come next. There is hardly, he says, a disagreeable, or even excruciating sensation that these patients do not experience, and too often this real suffering is mistaken for imaginary, because the patient's appearance does not correspond with her morbid sensations, as we are apt to observe them in other examples of disease, an error too frequently confirmed by the observation that these patients will often pass under a little excitement, from a state of excruciating suffering and loud complaints, to one of actual enjoyment and hilarity; or conversely, the excitement of private company, or a public party, give way to a doleful condition of suffering, or unappeasable complaints. Cephalagia in some form, either partial or general, Dr. Byford remarks on, as being very common, but persistent or frequently recurring pain in the occipital region, or on the summit of the head, is, he says, nearly pathognomonic of uterine disease. Affections of the spinal cord are also frequent, and sympathetic pains in the pelvic region, and in the bladder and rectum, but these latter may be the result of extensions of the disease rather than sympathetic. Affections of the sciatic and anterior crural nerves are also common, and there may be hyperesthesia and anesthesia of various parts, and sometimes spasms of the limbs and abdominal walls, going on, even to convulsions (hysterical). There may also be various manifestations of moral and intellectual perverseness, partial or semi-convulsions, partial paralysis of particular sets of muscles, derangements of the circulation and respiration, and of the excretions, mammary affections, and moral and mental derangement.

Having described all these affections, Dr. Byford closes the chapter with the following observations, which we extract at length :—

"I have dwelt so long on these general symptoms, and have made so much of uterine sympathies, that I am forced to recall an expression made use of in a notice of Professor Hodge's work on 'Diseases of Women,' that 'if all this is true, it is almost a pity that a woman has a womb;' but I conscientiously believe I have fallen very far short of mentioning all the sympathetic evils resulting from chronic diseases of the uterus, and I only design this as an outline view of a subject that will fill itself up in painfully warm colours in the observation of those who devote themselves to a close study of the diseases of women. While this is my conviction, I do not wish to be understood as saying that nearly all of the above symptoms will show themselves even in a majority of cases: some of them will be prominent in some cases, others in other cases; and in rare instances we meet with nearly all of them in some sufferer, and in nearly all chronic cases we shall find enough to move us to commiseration for the ruined health of women thus affected. I know there are thousands of my peers in the profession, who do not see in the foregoing array of symptoms any indication of disease of the uterus; and when uterine diseases are obviously co-existent, they are apt to be arranged in the order of sequency. This does not shake my faith in the facts I have observed for myself, nor disturb my judgment formed from an observation of a very large number of cases carefully watched through all stages of progress to their termination. That all the above symptoms may occasionally be present in cases in which the uterus is healthy, I have often observed; but that they are also frequently present as the proximate and remote effects of uterine disease, I am well satisfied. Another well-established fact, according to my judgment, is, that the direct symptoms referable to the uterus may be feebly pronounced, while some or even a large number of the sympathetic disturbances are very prominent; and judging from the freedom of pain and other inconveniences experienced in the uterine region, there are even cases in which the uterus does not seem to suffer at all. These cases are well calculated to mislead us, and to induce the opinion that the womb difficulty is of minor importance, and need not be the object of solicitude until we get rid of the more troublesome and prominent symptoms. We cannot be too careful in our consideration and management of this class of cases, and I insist, that while we adopt judicious remedial means for the removal of the more afflicting symptoms, that we be sure to address ourselves to the disease of the uterus, however slight it may appear to be. I have seen too much good result from the observance of this direction not to dwell with emphasis upon its importance. The cure of the uterine disease will be a valuable diagnostic measure in such cases. Not only

may there be a great difference or want of correspondence in the severity of the local and general symptoms, but in many cases in which the general symptoms have almost made a wreck of the health and happiness of the patient, the local inflammation and ulceration will be found upon examination to be trifling in amount and degree. The inflammation may be very slight and the patient suffer very greatly from it, either generally or locally, or both; or the ulceration may be extensive and the inflammation very considerable, and yet the patient hardly be sensible of any inconvenience whatever from its presence. This statement will be confirmed by careful observers in this field of research. It, however, will prove a stumbling-block to those who entertain the opinion that uterine disease is of small importance in the consideration of woman's ailments. They seem to think that there is of necessity an exact and invariable correspondence between the magnitude of cause and effect, and they point to these cases and say,—the symptoms were present, but very trifling, if any, uterine disease showed itself upon examination; or they will say, there was great ulceration, but the patient did not suffer from its presence, at least not in proportion to the amount of local disease. I need not particularize instances in which other diseases are comparatively latent, or cases in which the symptoms are unduly severe compared to the amount of actual disease, as they will suggest themselves to every intelligent practitioner. But recurring to the sympathies of the uterus, we find that while some patients are not affected at all by pregnancy and others are favourably affected, their health being better then than at any other time, that some absolutely perish on account of the same functional derangements inaugurated by pregnancy; and as is shown on a former page, organic diseases are not unfrequently lighted up. We will probably always be at a loss to understand precisely this difference; but there can be no doubt that it is more on account of constitutional differences than local ones. The concatenation of sympathetic influences may be caused by the greater susceptibility of the organs secondarily affected. In fact, the only mode of accounting for it is by supposing this increased susceptibility. I am convinced that this great but inexplicable diversity of sympathetic effects is as likely to result from uterine disease as from pregnancy. We must expect a very great range of difference in the extent of sympathetic derangement from uterine disease. It is interesting to observe the rise and development of the sequences to diseases of the uterus. How far can the uterus produce a direct effect in creating this large amount of sympathetic disorder? Are most of the symptoms produced by the direct sympathetic relation of the uterus to other organs, or does the diseased uterus first affect some other more influential organ detrimentally, and then this last the organism generally? I am inclined to think, from a large observation, that the uterus has direct sympathy with only a few organs, and no one probably is so powerfully affected by

it as the stomach. It is the first organ affected by it in pregnancy, being brought into a morbid condition in a very few weeks. The well-known, powerful, and almost universal sympathetic influence exerted by the stomach upon other viscera is sufficient, when it is diseased, to account for the great variety of subsequent symptoms. The stomach is the great centre from which radiate abdominal, thoracic, cerebral, and spinal disturbances almost *ad infinitum*. And there can be no reasonable doubt that it is an active agent in originating the disturbances of the great vital organs. The subject of the sympathetic influence of the uterus then becomes the more interesting and important, from the fact that very slight deviations from its ordinary condition arouses the most influential of all the organs to a state of disease which depresses the functional energies and increases the susceptibilities of almost all the rest of the organism. In addition to the chain of sympathetic susceptibilities produced by this state of the stomach, too frequently the digestive powers of that organ are impaired or perverted, so as to supply the chyme in deficient quantities or in deteriorated quality, and in this way injuriously affect the composition of the blood, inducing anæmia or oligæmia. Imperfect nutrition will follow, as a matter of course, in the one case, and perverted nutrition in the other, so that emaciation or obesity will be ordinarily present. Another organ probably in direct sympathy with the uterus is the cerebellum, as it seems to me to be as frequently affected as the stomach. The mammæ are, of course, in direct sympathetic relation with the uterus, and yet they are not uniformly affected in many cases when the uterus is very seriously diseased. I do not believe that we are able to say, at present, whether there are other organs that come directly under uterine influence. A proof of the powerful and very ready effect upon other organs of irritation of the uterus, may be found in the fact, that very often when the patient is in a condition of comfort, so far as her general suffering is concerned, an application of nitrate of silver to a morbid os uteri will give her excruciating pain in the head, render her exceedingly despondent and irritable, and very much aggravate the symptoms with which she is affected. This I have so often observed to be the case, that I cannot but regard it as one of our diagnostic means. After such an application, the patient will generally complain of an aggravation of the general symptoms, whatever they may have been, and say that all the pains are made worse by the application of the caustic. When an organ has been the subject of irritation or functional derangement for a long time in consequence of sympathy with the uterus, it may become the subject of organic disease, and then continue as an independent affection of perhaps a dangerous character; or if organic has not succeeded to functional disease, the power of habit which is so frequently thus engendered will perpetuate morbid action for an indefinite period after the cause of it has been removed."

Dr. Elliot's work is truly described in the title page as an "Obstetric Clinic: a Practical Contribution to the Study of Obstetrics, and the Diseases of Women and Children." It is a record of the practice of an experienced and thoughtful physician. The cases are detailed in such a manner as to bring them most fully and fairly before the mind of the reader, and the practice pursued is faithfully reviewed from time to time, and the reasons which led to the adoption of particular methods are explained and criticized; the author not hesitating to show, when necessary, how his further experience might have induced him to adopt other methods. Puerperal eclampsia is the first subject considered; the section opening with a chapter on the relations of albuminuria to pregnancy. We cannot help regretting that Dr. Elliot has kept so strictly to the role he had assigned to himself of practical observations when dealing with this subject. There are so many difficulties surrounding the pathology of puerperal convulsions that we would have been glad to have learned the conclusions arrived at by one who had so closely studied the subject. That puerperal convulsions and albuminuria are most intimately connected is patent to every one, but the nature of the connexion is not yet known, though we have made some progress, in so far as we have cleared away certain theories that for a time represented all our knowledge, but have been proved, by subsequent research, to be erroneous. When Dr. Lever and M. Rayer drew attention to the presence of albumen in the urine of women suffering from eclampsia, it was thought we had in this the cause of the convulsions, and that as albuminuria is a symptom of a fatal disease of the kidneys, in which convulsions frequently occur, so it indicated a similar condition of these organs as the cause of puerperal convulsions. It was soon found, however, that the albuminuria of pregnancy was not permanent; that it was not always associated with convulsions; and that convulsions may occur independently of it. And it is now known that we may have albumen in the urine under different circumstances, viz.:—as a consequence of a superabundance of this material in the blood, a condition that seems to be natural in pregnancy, or from over distention of the blood-vessels of the kidneys from the pressure of the enlarged uterus on the emulgent veins, or from a diseased condition of the kidneys themselves. Associated with albuminuria a deficient excretion of urea is generally found; and puerperal convulsions have been attributed to the retention of this substance in the blood,

or uremia; but further observation proved that urea might exist in the blood without giving rise to convulsions, and it was then suggested by Frerichs that there must also be some peculiar ferment present to convert the urea into carbonate of ammonia. Ingenious as was this theory, it too has had to give way, as it has been amply proved that there is no necessary connexion between convulsions and this state of the blood. The same may be stated of the condition described by Gubler as "urinemia," by which is meant a condition in which not only urea but various extractive matters are retained in the blood. Then again Traube suggests that uremic convulsions are due. not to the presence of the constituents of the urine in the blood, but to œdema and anemia of the nervous centres. The drain of albumen by the kidneys, by lessening the density of the liquor sanguinis, makes it, he says, more prone to transude, especially where the tension of the vessels is increased by hypertrophy of the heart, a condition without which uremic attacks do not, he says, occur; and he further states that these attacks are preceded by anemia and pallor, and that the brain is found in fatal cases anemic and œdematous. We are not aware that hypertrophy of the heart has been observed in connexion with puerperal convulsions, but it is quite possible that the increased tension caused by pregnancy and labour may supply the conditions due to the increased cardiac action under other circumstances. But we have already observed that puerperal convulsions may occur where there is neither albuminuria nor uremia; and Dr. B. Hicks has shown that in some cases the occurrence of convulsions precedes and seems to give rise to the albuminuria, or, at least, that the two conditions may depend on the same cause. Dr. Hicks suggests that this cause may be some peculiar condition of the blood, but it cannot be questioned that the convulsion may in some cases depend on a reflex irritation of the spinal cord, as suggested by Scanzoni, and that this centre may itself be in a state of hyperpolarity, as suggested by Dr. Todd. Though Dr. Elliot does not treat of these subjects directly he records many facts bearing on them. and of great interest, not only for this reason, but for their more directly practical importance. The first case he gives is that of a primipara, aged 21, who had œdema, vomiting, disturbance of eyesight, and whose urine was loaded with albumen, and had a sp. g. of 1018, and contained hyaline, epithelial, and granular casts. This woman died four days after delivery from peritonitis, when the kidneys were found in an advanced stage of fatty granular

degeneration, and yet she never had any symptom of convulsion.

The second case is more remarkable still. It was the first pregnancy, and not only was there œdema, but the urine became almost solid when tested by heat and nitric acid, and not more than one or two ounces were passed for several days; labour was very tedious, extending over four days, notwithstanding five applications of the douche to the os, and the use of Barnes' bags, to effect dilatation, and the child, which was dead and putrid, presented by the breech, and required considerable effort for its extraction, owing to the enlarged and swollen state of the head. This woman made a good recovery; never had a convulsion, and was dismissed from the hospital at the usual time. Speaking of these cases, Dr. Elliot refers to the condition of the kidneys and blood as the cause of convulsions, and proceeds to detail the results of the observations made at Bellevue Hospital, where it appears it is the rule to examine the urine of all pregnant women admitted into the waiting wards. Of 437 cases, albumen was found in the urine of 23. In three of these, however, it depended on the presence of pus, and deducting these it appears albumen was found in the urine of 1 out of every 22 pregnant women, and of these only 1 is known to have had convulsions. In one case, where there was no albumen before labour, convulsions occurred, and albumen was then found as in some of Hicks' cases. Two of the women who had albuminuria left the hospital before labour and were lost sight of. These facts sufficiently prove the occurrence of albuminuria without convulsions, but the converse, or the occurrence of convulsions without albuminuria, is not so well established, partly, perhaps, because the convulsions may cause albuminuria, as in the cases referred to above; but, independently of this, the cases recorded by Depaul, Mascarel, and others, seem to place the matter beyond a question; and Dr. Elliot records four, in two of which at least, Nos. 30 and 38, the convulsions were of the true epileptiform character, though in the others, 19 and 20, they may possibly have been hysterical.

We shall not, however, dwell longer on this subject, attractive though it be, but turn to Dr. Elliot's remarks on the treatment of puerperal convulsions, which are so valuable as to more than compensate us for any deficiencies in the expression of his views on the pathology of the disease. He first speaks of prophylaxis, which he sums up as follows:—"Eliminate through the bowels, the skin, and, perhaps, the kidneys; consider the advisability of inducing

labour, and of abstracting blood; diminish the supply of meat and indigestible food; remove exciting and depressing influences; do not debilitate the patient; ward off threatening attacks with chloroform or sedatives." Dr. Elliot speaks most favourably of the prophylactic virtues of mercurials in minute alterative doses, especially of corrosive sublimate, which may be combined with bark or iron. Of purgatives, Dr. Elliot prefers the salines, but occasionally uses elaterium and other more active ones. Diaphoresis he attaches great value to, and uses a portable hot-air bath for inducing it. He concurs with those who say no man ought to expect health who has not been thrown into a good perspiration at least once a day. Diuretics also he does not hesitate to use; and as for the specific remedies which are said to neutralize or counteract the poison in the blood, such as benzoic acid, lemon juice, and tartaric acid, it is better, he says, "that they should be given, especially the latter, as they will not do harm, and are claimed on high authority to be efficient." Unless meat be obviously necessary he forbids its use almost entirely. The induction of premature labour is another valuable means of prophylaxis. On this we shall allow him to speak for himself.

"*The Induction of Labour as a Prophylaxis.*—From the preceding remarks, it is evident that there is no absolute physical sign, on which we can alone rely, to decide the question whether premature labour should be brought on or not. The decision rests on the summing up of the whole case; and the operation is therefore to be postponed, if possible, unless the term of pregnancy be already reached, when we are entitled to a larger liberty, with the present excellent methods at our command.

"As in all elective operations, we are liable now to regret that it has been postponed too long, and then to question whether it might not better have been deferred. Unless the decision be arrived at on account of existing eclampsia or other serious contingency, if the result be not satisfactory, hostile criticism will not be withheld. And if it be satisfactory, and both lives be saved, it may be suggested that the operation was unnecessary, meddlesome, and hazardous. Such is the world; and hence the question should be decided in deliberate consultation, and the burden divided among strong shoulders.

"My experience leads me steadily to look more favourably on the induction of premature labour, in cases where repeated examinations of the urine show that the conditions of disease are persistent, or progressing despite treatment; where some of the prodromata of eclampsia—the first mutterings of the coming storm—are present; and where the fetus is fully viable. In cases where the fetus is not viable, we must wait until

the mother's life is actually endangered by successive attacks of eclampsia, or for other conditions recognizable in consultation, which lead irresistibly to the clinical conviction that either the pregnancy or the woman's life must be terminated.

"In the following case the question of premature labour received my most serious consideration and it was postponed because the patient seemed to improve; a fact which I have since regretted."

In the treatment of convulsions chloroform has found more favour in Dr. Elliot's eyes than it has in this country. "If only one method of treatment were given to me for these cases," he says, "my choice would unhesitatingly be for chloroform. The chief indications are to terminate the labour as speedily as may be justifiable, and meanwhile to keep the patient moderately under the influence of chloroform." As to the relative merits of chloroform and sulphuric ether, Dr. Elliot expresses no opinion. He has used chloroform more frequently than ether, but if there were cardiac disease, or a tendency to syncope, he would use ether, simply because the majority of American practitioners think it safer; but where there is a tendency to debility, the administration of either should be preceded by a stimulant.

There has been so much difference of opinion as to the value of venesection, that we must again allow Dr. Elliot to speak for himself, and in so doing take our leave of this section of his work:—

"*Venesection.*—We are tempted to use venesection in cases of puerperal eclampsia by the recommendations of authors, and of so many practitioners—by clinical traditions in short—as well as by the appearances of great congestion observed in the head and face during the progress of the attacks. The purple, livid face and lips, and tongue; the congested conjunctivæ; and the duskiness of the skin, which is often observed; suggest abstraction of blood as a measure of relief. If we analyse our apprehensions, however, we find that extravasation of blood upon the brain is what we chiefly dread; and if we examine the records of autopsies we find that such a contingency is extremely infrequent, and probably associated in the majority of cases with fatty degeneration of the blood-vessels of that organ.

"Nor can it be said that cerebral extravasations are infrequent because of venesection, for statistics show that we may assert the fact in cases where this treatment has been discarded.

"Moreover, it may safely be said that a majority of the severe cases of eclampsia occur in patients who are anemic, and whose subsequent histories display tendencies to hydremia, and that a roborant treatment with iron is most generally indicated after the immediate dangers of

the confinement shall have passed. It is obvious that copious abstraction of blood, during the progress of these attacks, must therefore unfavourably influence the future convalescence of many of these patients.

"Another and natural argument for venesection at the present day, may be found in the likelihood that it may remove some of the poisonous principles which are supposed to affect the nervous centres; but we may eliminate these by other channels, and meanwhile powerfully control their influences by chloroform, until we shall have terminated the labour.

"Still, in patients of a plethoric habit, and more especially in cases where we suspect that atheromatous degeneration may be present, the moderate abstraction of blood may be a judicious practice, and not liable to do harm. But large and repeated bleedings do not seem to me to be indicated. Where there are evidences of anemia, the abstraction of blood should be resorted to with the greatest hesitation.

"In a great majority of my cases blood has been taken, though rarely by venesection, and in moderate quantities. In most wet cups have been used, a process applicable in various parts of the body, which adds the advantage of counter-irritation to the treatment, and measures accurately the amount of blood which is withdrawn, without the risk, so common in venesection, of taking more than is desired.

"I find myself resorting less frequently to this practice even, or with less confidence in the abstraction of blood in each succeeding year. In some cases it has seemed to me that the application of dry or wet cups over the kidneys has favourably influenced the action of diuretics."

We pass next to Chapter V., which treats of ante-partum hemorrhage, and which we stop to notice chiefly because of the evidence it affords of the wisdom of inducing labour by the use of Barnes' bags in cases of placenta previa, or accidental hemorrhage. We have seen by the American journals that this practice has been adopted by several of our transatlantic *confrères*, and it has long been advocated by Dr. Kidd in this city. We are convinced that it is the safest course to pursue, and that though many cases of ante-partum hemorrhage go on well without interference, it is infinitely better for both mother and child, where the pregnancy has advanced sufficiently far to render the child viable, to adopt such a prompt and decisive measure, than to run the risk of renewed attacks of hemorrhage, which may speedily end in the death of both.

Chapter VI. is on the induction of labour, and very fully discusses the various methods of so doing, but does not present anything requiring us to dwell on it. We therefore pass on to the seventh,

which is "on the effect of the tonic circular contraction of a band of external fibres on labour—brow and face presentations, and rupture of the uterus." Three cases are given, in which labour was obstructed by this irregular contraction of a circular band of fibres, and Dr. Elliot thinks such cases are often not appreciated, and are more frequent than is generally supposed. Two of the cases were brow presentations, and in the third the pelvis was contracted in its conjugate diameter. In each case the waters had been a considerable time evacuated, and in two of them the labour pains had almost ceased, so that it is probable the uterus had passed into the state of persistent contraction, recently described by Dr. Braxton Hicks. The state described by Dr. Elliot seems to us to be a form of this persistent contraction, well known to us all as occurring in cases of transverse presentation, when the waters have been long evacuated, and when the uterus becomes moulded, as it were, to the child's body; but we do not remember having met before now with any description of this peculiar condition in cases of head presentation. In each case delivery was attempted, first by the forceps, or vectis, and then by version, and on introducing the hand the contraction was discovered encircling the child's neck or body. In each case it was found necessary to have recourse to embryotomy, in one the cephalotribe was used (Scanzoni's), and in the other the head was extracted with the crotchet.

The next four chapters are on operations in deformed pelvis, and are of great interest. The last of them is on version, and in it Dr. Elliot, in comparing this operation with the use of the forceps, expresses a preference for the forceps, but he sums the matter up in this way:—"In a conjugate diameter of three inches and upwards, with a living child and a head presentation, my first choice would be for the forceps. Between two and a half and three, if the child were living, I should perform version;" and in this opinion we fully coincide.

The remaining chapters, five in number, are devoted to various subjects in connexion with the diseases of women and children, including prolapse of the funis. Our space will not allow of our dwelling on them. We have, however, said enough to justify our opening remarks, and to prove that the work is one of rare value, and of surpassing excellence.

Dr. Hewitt's is also a systematic treatise, and when the first edition appeared we noticed it in most laudatory terms, and we

are gratified to find the verdict we then gave confirmed by the appearance of a new edition. On two points only, did we venture to find fault—one being the arrangement adopted, and the other the want of illustrations—and both of these are corrected in the present edition. The pathology of the diseases of women is more systematically entered into, and is considered in conjunction with the treatment, and the illustrations are now numerous and admirably designed and executed. We have always, however, valued Dr. Hewitt's book most for its teachings on diagnosis. Half the book is still devoted to this subject, and an analytical system is adopted which not only ensures fulness, precision, and accuracy of observation, but trains the mind to the adoption of method, the only way by which success is to be obtained in the diagnosis of disease. We again commend this book to all, but especially to students.

The character of Dr. Tilt's work is so well known that we scarcely think it necessary to do more than inform our readers that the third edition has for some time been in our hands. That it has met with the approval of the profession is evident from its having so quickly arrived at this stage of its existence, and having been reprinted in America, and translated into German and French. The present edition is considerably enlarged, and yet retains its characteristics of conciseness and brevity. There are no cases that require so much tact and delicacy, so many changes and alterations, and at the same time so much steady perseverance in following the one main course of treatment, as chronic uterine diseases; and to have such a book as this of Dr. Tilt's to refer to under such circumstances is almost indispensable. We constantly use it for such purposes, and always with benefit and satisfaction. We must, however, caution our readers that it is not intended to be a complete system of uterine medicine and surgery, any more than a work on Materia Medica is supposed to contain all that is required to be known on the Practice of Physic. There are, moreover, many suggestions in it that require to be taken with caution, and not a few that must be regarded as puerile; but notwithstanding all this we commend it to all who undertake the treatment of the diseases peculiar to women, and we have no doubt they will find it of great use to them in many an emergency.

We are indebted for the translation of Klob's work to Drs.

Kammerer and Dawson, of New York. The volume before us is the first part of Dr. Klob's book, and treats only of affections of the uterus. It is divided into sections treating of—I. Anomalies of formation, including those of fetal development, by alterations in quantity and quality, and anomalies of situation; II. Anomalies of uterine development during childhood; III. Anomalies of formation in the latter part of exra-uterine life, including anomalies of formation altered in quantity and in quality. Under this section we have anomalies of site, including flexions, versions, prolapse, &c., alterations of the form, and adventitious growths. In an appendix to this section we have entozoa and entophyta. The next section is on anomalies of nutrition, in two sub-sections, one on quantitative alterations of nutrition, such as hyperemia, hypertrophy, atrophy, ulcerations, wounds, &c.; and one on qualitative alterations of nutrition, of which two species are given, fatty and amyloid degeneration. Puerperal affections of the uterus is the next and concluding section.

It will be seen from this sketch that the work embraces the whole range of uterine pathology; but it is a difficult book to read, both from the style and the peculiarity of the views of the author, and after a somewhat careful studying of it we have felt not a little relieved on turning to Dr. Farre's article on the uterus, in the *Cyclopedia of Anatomy and Physiology*, and finding there pretty much the same information in a much more accessible form.

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1. *The Practice of Medicine.* By Thomas Hawkes Tanner, M.D., F.L.S., Member of the Royal College of Physicians; Fellow of the Royal Society of Literature. In two Volumes. The sixth Edition, enlarged and thoroughly revised. Henry Renshaw, London. 1869. 8vo, pp. 656 and 645.

2. *A Text Book of Practical Medicine, with particular reference to Physiology and Pathological Anatomy.* By Felix Von Niemeyer, Professor of Pathology and Therapeutics; Director of the Medical Clinic of the University of Tübingen. Translated from the seventh German edition by special permission of the author, by George H. Humphreys, M.D., and Charles E. Hackley, M.D., New York. 2 Vols. New York: Appleton and Co. 1869.

For many years we have had constantly in our hands Dr. Tanner's

"Practice of Medicine," and been in the habit of recommending our pupils to use it as their text book. We have read the chapters in the present (the sixth) edition, which treat of subjects on which of late we have obtained increased knowledge, and have satisfied ourselves that the work has been carefully written up to the present time. The author is thoroughly familiar with the literature of the profession, he has digested and made his own of the views of others before he attempted to reproduce them, and he writes smoothly and well; the result is that the work contains an accurate and clearly written account of the received pathological doctrines of the present day; but Dr. Tanner being himself actively engaged in dealing with the emergencies of practice, has not lost sight of the art in his exposition of the science of medicine; on the contrary, ample space is devoted to the subject of treatment, embracing not only the administration of drugs, but all the important details of general management. We believe the student will find the work the best text book on the Practice of Medicine, while the practitioner will possess in it a thoroughly safe guide at the bed-side. In the appendix, moreover, is contained a vast amount of information on the feeding of the sick, on climates and mineral waters suitable for invalids, and on the modes of prescribing and combining medicines, information, which, as far as we know, is not elsewhere accessible. The book, which is published by Mr. Renshaw, has been beautifully printed, and now appears in two handsome volumes.

Drs. Humphreys and Hackley have rendered important service to the English-reading profession by their excellent translation of "Niemeyer's Treatise on Medicine." The task of a reviewer is an easy and gracious one in noticing such a work as this. It is at once comprehensive and concise; it is characterized by clearness and originality, and it differs from many German works on medicine in the sagacious appreciation of the value of therapeutics which is manifested by the author. It has been often, and justly objected to German physicians, that their attention is absorbed by diagnosis and pathology, to the exclusion of any adequate efforts at remedial measures. Niemeyer, while sharing to some extent in the sceptical spirit of his countrymen, is, however, to a great extent, free from this reproach, and accordingly his book will be found more useful to practical men, than any other of the standard German works on medicine with which we are acquainted. In many instances the views adopted by the author are at variance with those entertained

in this country, and on a considerable number of points we should be compelled to differ from him rather widely, but we can cordially recommend the work as one from the perusal of which every physician may derive profit, which will be found replete with information of direct practical value, and which is instinct with a spirit of candid and earnest inquiry, which is worthy of all praise.

The translation, on the whole, is very creditably performed, and the volumes are handsome and well brought out.

On Chronic Bronchitis, especially as connected with Gout, Emphysema, and Diseases of the Heart. Being Clinical Lectures delivered at the Middlesex Hospital. By E. HEADLAM GREENHOW, M.D.; Fellow of the Royal College of Physicians, &c. London: Longmans, Green, and Co. 1869. Fcap. 8vo, pp. 236.

EVER since we read Dr. Greenhow's paper on "Cholera," published in 1851 in the *Medico-Chirurgical Review*, we have entertained the highest respect for him as an accurate observer and acute interpreter of pathological facts, and it was our intention to give a pretty full account of the volume before us, an extended notice of which was indeed ready for our last number, but was omitted for want of space. We find ourselves similarly circumstanced on the present occasion, and unwilling to postpone any longer drawing our readers' attention to the work, we must give it a shorter space than otherwise we would wish to do.

Dr. Greenhow has taken up the idea that bronchitis, in a large proportion of cases, acknowledges the gouty dyscrasia as its predisposing cause, while exposure and the inhalation of irritating matters are merely the exciting causes. In thirty-four of ninety-six persons whose cases he noted for the purpose of this inquiry, he found a distinct history of gout attaching either to the patients themselves, and to some of their immediate relatives; and he seeks indirectly to support the same conclusion by showing the frequency with which bronchitis occurs in persons who are likewise sufferers from eczema, psoriasis, or renal disease, all of which he looks upon as evidences of the gouty constitution. Those of our readers who desire fairly to appreciate the force of Dr. Greenhow's statistics must consult the work itself. Great as is our respect for him, we confess that we think he has, in this instance, got hold of a wrong

thread. No doubt in London there are many martyrs to gout who have also bronchitis, but that the latter is an expression of the former in any but very exceptional cases we do not believe; there are few practical physicians who have not occasionally seen an attack of bronchitis subside on the supervention of gout, and now and then a cough, which other remedies failed to cure, will yield to colchicum, but the same may be said of bronchitis, malarial poisoning, and quinine. In this city bronchitis stands far ahead of every other ailment in its frequency among our hospital cases, but gout is so rare that a man may be physician to a large hospital and not have a case to show his class in twelve months.

Our observations are more in accordance with those of Dr. Greenhow on the pathology of emphysema. In a certain proportion of cases, he believes the lungs become emphysematous without any bronchitic attack, or other cause of over distension of their vesicles, while in many instances the catarrh which produces the emphysema is so slight as to be quite insufficient to injure a healthy lung. In other words, he believes the first step is degeneration of the walls of the air vesicles themselves from some constitutional vice. Acting on this view, he has been in the habit of administering, and, he believes, with excellent effect, the syrup of the iodide of iron during the intervals between the bronchitic attacks. Otherwise, on the subject of treatment, we have not noticed anything special. In bronchitis, with profuse expectoration, he has found the tincture of larch very valuable, and free from the nauseating effects of the resinous drugs usually administered in such cases.

The last two chapters contain a clearly written account of the connexion between bronchitis and emphysema on the one hand, and cardiac disease on the other.

The Mechanism of Dislocation and Fracture of the Hip, with the Reduction of the Dislocations by the Flexion Method. By Henry J. Bigelow, M.D., &c.

This monograph, with the monosyllable title "Hip," is chiefly occupied with a discussion of the mechanism of dislocation of the head of the femur, the determining causes of the position of the head of the bone in the various forms of dislocation, and the manner in which they should be reduced. The mode in which the reduction is effected is explained by the author's conception of the

pathological condition during dislocation of the component and surrounding parts connected with the joint. The latter part of the book treats of fracture of the neck of the thigh bone, and contains much valuable scientific and practical information.

The idea certainly largely prevails, that in dislocation of the thigh the chief obstacle to reduction is the action of the muscles, and that they determine the position assumed by the dislocated femur. It is also generally assumed that the capsule is largely, generally completely, torn through. The author's task is to combat both these propositions. He alleges that muscular action is little or no impediment to reduction, and that the position assumed by the head of the bone in the ordinary forms of dislocation is determined by the anterior portion of the capsule, which usually remains intact, and also by the obturator internus muscle with the portion of the capsule lying subjacent to it, the peculiar formation of this muscle rendering it of very great strength.

The ilio femoral ligament is carefully described by the author, who points out that above it arises from the anterior inferior iliac spine, whence it spreads out to be attached into almost the whole of the anterior introchanter ridge. Its insertion, on being examined, is found to consist of two principal portions, anterior and posterior, much thicker than the rest, so that the form it assumes, and by which it is described by the author, is that of an inverted Y. The writer of this notice has verified this anatomical arrangement for himself, and after the rest of the capsule and the muscles around the hip have been cut, the thigh bone may be made to assume the positions it takes in the usual forms of dislocation just as the author describes. The strength, too, of these fasciculi forming this Y ligament is very great, and the author mentions that its breaking weight varies from 250lb. to 750lb. He concludes that—

"Although some autopsies show that the whole of this ligament may be torn asunder, it may be assumed that such a lesion is likely to be of rare occurrence. Its strength, probably, insures its immunity in a large majority of luxations, while the constancy of their signs, which will be shown to be best explained by the action of the ligament, testifies to its integrity."

The author divides the luxations of the bone on to the ilium into those above and those below the obturator internus tendon, those below being the ischiatic of Sir A. Cooper, and he denies that there is any special difficulty in reducing what is usually called dislocation into the sciatic notch if it be properly attempted.

"It has been before remarked, that when the flexion method is universally adopted it will be practically needless to classify separately the dorsal luxations. Their varying relation to the small rotator muscles has, however, been already shown, and the strength of one of these muscles may entitle it to separate considerations.

"The dislocation hitherto distinguished as 'upon the ischiatic notch,' and unnecessarily associated with it, is characterized by Sir Astley Cooper as differing from the dorsal displacement chiefly in producing less shortening of the limb. I believe that no dislocation upon the ischiatic notch is worthy of the name, that no satisfactory or practical result can be based upon this distinction alone, and that it is also an error to suppose that during reduction the femur ever notably 'slips into the sciatic notch,' or that the sciatic notch ever offers any obstacle to its reduction. A little more or less shortening and a varying degree of inversion depend both upon the position occupied by the head of the femur upon the dorsum and on the degree of laceration of the capsule. In cases of this variety which have been recorded, the signs were intrinsically the same, and reduction, if by pulleys, was usually effected in one and the same way, unless we seek an exception to this statement in a slight variation of the angle of traction, quite as likely to occur in one case as another, and even to vary much in different attempts upon the same patient."

All backward dislocations are reduced, the author states, with the utmost facility, whether on the dorsum ilii or lower down, by means of flexion, abduction, and rotation outwards. In the forward luxations—viz., those on the pubis and thyroid foramen—the converse rule obtains. Flexion, and what the author styles angular extension, are what he chiefly relies upon in the reduction of nearly all dislocations. The angular extension is effected by buckling the patient's pelvis immovably to the floor, and then pulling on the thigh in a vertical direction by means of pulleys or otherwise. In this way, while flexion is maintained, the extending force may be applied, and the limb at the same time rotated either outward or inward.

Whilst reading this book we were struck by the corroboration of some of the author's views, afforded by the particulars of a case of dislocation of the femur brought forward by Mr. W. Adams at a recent meeting of the Pathological Society of London. The patient had died from the shock of severe railway injuries, with an unreduced luxation of the hip directly backwards, which afforded the somewhat rare opportunity of dissecting the parts concerned in a recent dislocation of the hip. The head of the bone rested on

the base of the spine of the ischium, having passed out between the obturator muscles, and being prevented, Mr. Adams states, from passing further upwards and backwards by the tendon of the obturator internus muscle, which was tightly stretched across the head of the bone. In fact, a very similar condition is figured in one of Dr. Bigelow's beautifully executed plates on page 65. Mr. Adams further describes, that the obturator externus, quadratus femoris, and ligamentum teres, were all ruptured, and the posterior half of the capsular ligament. The strong anterior half of the capsule evidently formed the chief obstacle to reduction, as it could with difficulty be stretched enough, when the great trochanter was everted, to allow the head of the bone to be raised over the posterior prominent border of the acetabulum. The movements required to effect reduction after death were, first, extension of the thigh in an oblique direction inwards, and then powerful rotation outwards of the thigh on its own axis. The head of the bone was by those means pulled into the socket by the stretched anterior portion of the capsule. It did not appear, adds Mr. Adams, that any important muscular resistance could have been offered to the reduction of the dislocation. Now this is most valuable, because perfectly independent, testimony to the truth of Dr. Bigelow's views, and there is no doubt that had the thigh been flexed and abducted as the author recommends, and then rotated outwards, the head of the bone would have been much more readily tilted into the acetabulum, and not have jammed against the posterior edge of it, as from the above description it would appear to have done.

We cannot too highly praise this book as the work of an accomplished and scientific surgeon. We do not hesitate to say that he has done much to clear up the obscurities connected with the mechanism of dislocation of the hip-joint, and he has laid down most valuable practical rules for the easy and most successful management of these injuries. The monograph is largely illustrated with exquisitely executed woodcuts after photographs, which help to elucidate the admirable subject-matter of the text. We cordially commend the "Hip," by Dr. Bigelow, to the attention of surgeons.

The Body and its Health: a Book for Primary Schools. By E. D. Mapother, M.D., Professor of Physiology, Royal College of Surgeons; Surgeon to St. Vincent's Hospital, &c. Twenty-one illustrations. Dublin: John Falconer, 53, Upper Sackville-street. London: Simpkin, Marshall, and Co. Edinburgh: Menzies. Melbourne: Robertson. New York: Harpers. Pp. 128.

Dr. Mapother has been known for many years to possess the rare gift of communicating anatomical and physiological knowledge to those whose minds were often little fitted for catching and holding the facts of a new science. In this he has had much experience, and we were not, therefore, surprised to find in the little work before us the great phenomena of life stated in such a way, in plain homely language, that any intelligent boy or girl may understand them. On the advantage of putting such knowledge as this into the hands of the public we have often insisted; and we therefore cordially welcome this, which might well be entitled the first step in knowing ourselves.

Traité de Paléontologie Végétale, ou la Flore du Monde Primitif dans ses rapports avec les Formations Géologiques et la Flore du Monde Actuel, avec un Atlas de 100 *planches.* Par W. Ph. Schimper. Paris: Bailliere et Fils. 1869; tome premier, pp. 738.

When a cryptogamist of the world-wide reputation of M. Schimper bestows on the world a treatise on the obscure science of Palœophytology, it leads us to expect that the work will be a most valuable contribution to the literature of this subject, which is truly characterized by one of the greatest of living botanists as "the most unreliable of sciences." And indeed the present work fully comes up to our expectations, and is most thorough and exhaustive as far as it goes, recording all that has been done and written in the domain of fossil botany since the days of the old standard works of Brongniart, or Lindley and Hutton.

Perhaps no branch on natural history has been making so rapid progress of late as fossil botany. The labours of Binney and Carruthers among Calamites, of Heer among the Miocene leaf beds of

Œningen and Disko, and of Saporta, Lesquereux, Schimper, and others, have accumulated immense quantities of facts, which are well systematized in the work before us. In this, the first volume, the talented muscologist of Strasbourg gives us his introduction and the first part of the botanical classification of fossil plants. The introduction consists of a series of discourses on the mode of preservation of fossil plants, the secular changes in the vegetable kingdom, and the application of Palæophytology to Geology and Climatology, &c. He appears before us as an able defender of the doctrine of evolution, and one whose opinions are of the more weight because he is universally acknowledged as *facile princeps* in the special domain of muscology; for when eminent specialists give forth opinions on the genesis of species, based on the study of the particular branches to which they have given their attention, far more weight attaches to this expression of belief than to the testimony of mere general or of dilletanti naturalists; and hence the opinions of such men as Fritz Müller, deduced from the study of crustacea, or of Schimper, are worthy of being regarded as of great importance in this controversy on the origin of species.

Professor Schimper has introduced an innovation in botanical terminology, which is of value—namely, changing the affix of the names of some fossil genera from ites to ides. The latter, he says, expresses only resemblance, the former relation to living forms after which they are named. Now, if they are related, no special term is needed, and the ordinary botanical term should be used; but in the majority of fossil genera we have not sufficient grounds upon which to pronounce positively on relationship; hence the innovation is an improvement.

In his arrangement of ferns he has carefully revised the entire group, and has made many improvements in grouping, and in the arrangements of genera. Among these improvements we notice he has founded a new genus Palæopteris for the reception of the Devonian Cyclopteris Hibernica, and its allies the C. Römeriana of Moresnet, the C. Bockschii, the Spheropteris casa of Hall, and the Asplenites Reussii of Ettingsheimer.

In his revision of the Calamites, he has adopted many of the improvements in classification based on our more extensive knowledge of the structure of the group. In conclusion, we may state that by endeavouring to place the obscure science of fossil botany on a true philosophical basis he has earned the thanks not only of the geologists and botanists, but of all the scientific men of the present

day. The Atlas consists of fifty plates, which are executed carefully and accurately. and they form, undoubtedly, the best series of illustrations of fossil plants yet published. They will be of immense value to the practical geologist, as affording an assistance to the ready recognition of species which has been hitherto wanting.

Les Anomalies dans le parcours des Nerfs chez l'Homme. Par W. Krause et J. Telgmann. Traduit par S. H. De La Harpe. Paris: J. B. Bailliere et Fils. 1869. Pp. 76.

It was remarked by the great anatomist Meckel that the nervous system is not liable to many variations, and certainly when we contrast the frequently varying muscular, osseous, and vascular systems with the nervous, we must be struck with the fact of the greater constancy of the latter to its typical arrangement. Still, however, anomalies do occur in the disposition of nerves, and a knowledge of some of these might possibly help the physician, in some cases, of obscure nervous affections, to explain anomalous symptoms and unusual sympathies. Scattered through the various anatomical journals, numerous instances are given of nerve anomalies; and Professor Krause and M Telgmann have carefully collected these, and placed them in a convenient form for reference. The work is very well compiled, clearly indexed, and forms a very useful addition to the library of the practical anatomist.

The World as Dynamical and Immaterial, and the Nature of Perception. By R. S. Wyld, F.R.S.E. Edinburgh: Oliver and Boyd. 1868. Pp. 227.

The words of the poet, "Things are not what they seem," have been adopted as a principle by many philosophers of the present day, while its contradictory that things are only what they seem, and nothing else, may be taken as embodying the main views of the philosophers of the positive school; and in this controversy of thought new versions of old ideas, and even old, dilapidated, and effete doctrines themselves, are constantly being raked up and presented to the reading public in new dresses. In a recent number of this Journal we had occasion to refer to some of these

philosophic systems in connexion with the theory of teleiotic ideas propounded by Professor Laycock, of Edinburgh, in his work on "Mind and Brain." The author of the book now under review, taking his stand in a more remote corner of the world of ideal philosophy, has brought out the new theory of Pyrrhonic ideality, and strives to demonstrate that there is no matter. Reviving the old Berkeleyan argument of the indemonstrable nature of matter, and arguing that we have no evidence of the existence of anything but force, he deduces from the universally admitted "law of parsimony in nature," that no matter exists; for if matter did exist, it could only be conceived of as an inert, an unenergizing substance, and therefore unnecessary, and therefore non-existent.

This is the main principle brought out by Mr. Wyld in his book, which is divided into two parts, the first dealing with the nature of of the world, and the second with perception.

For substance, Mr. Wyld would say collection of atomic centres of force, and to the theory that each of these atomic centres has a material basis, our author says:—

"If the forces, of whose existence we are alone conscious, are the important and efficient properties observed in a physical world and are chemical atoms, what necessity is there to assume the existence within the centre of force of an inert and insensible part, which furnishes no explanation of physical phenomena which is never seen—which is never felt? Is it not irrational to suppose a thing exists which explains nothing? Is it credible that a wise Being should create a thing which is of no use, and which thus requires to be bolstered up?"

On further inquiry into the source of power, the author arrives at the conclusion that it is the "attribute of an intelligent spiritual being," and not of an unconscious thing; and while thus characterizing power, he declares that his theory has none of the dangerous features of Pantheism; but if every physical phenomena be a manifestation of power, and if power be an attribute of an intelligent being, then physical phenomena are manifestations of the attribute of an intelligent spiritual being, but they can, in the nature of things, have reference to no being but God; for, if it be true, to whatever being these phenomena are attributed must be God, the conclusion is obvious that the theory, as expounded by Mr. Wyld, is one of pure Pantheism.

The theory of perception adopted by Mr. Wyld is what might be expected from his theory of the world; and altogether his book

is thoughtfully written, although we believe that its foundation is as airy and substanceless as Mr. Wyld's world. The employment of the fallen spirits, as described by Milton, in discussing deep, abstruse, and incomprehensible subjects—"fixed fate, free will, foreknowledge absolute"—and such kindred topics, is one often indulged in by some philosophers; and in their cases, as in those described by Milton, the speculators soon are immersed "in wandering mazes lost," and Mr. Wyld, though scarcely so ideal as some other authors who adopt the German style of thought of the school of Hegel, yet is rather transcendental for the majority of the students of natural and physical science of the present day.

PART III.

QUARTERLY REPORTS.

REPORT ON SURGERY.[a]

By Edward H. Bennett, M.D. and C.M., T.C.D., F.R.C.S.I., M.R.I.A.; Surgeon to Sir P. Dun's Hospital; University Anatomist and Lecturer on Clinical Surgery in the School of Physic.

The treatment of carotid aneurism is thus described in the last edition of Mr. Erichsen's Surgery, published during the past year:—

"Since the time when Sir A. Cooper first ligatured the carotid, in 1805, the only means on which the surgeon relies for the cure of aneurism of this vessel is deligation of the artery at a distance from the sac. When the aneurism is so situated that a sufficient extent of healthy vessel exists between the sternum and the base of the tumour to admit of the application of a ligature, the Hunterian operation may be practised. If, however, the root or lower portion of the artery be so involved that there is no room to apply the ligature between the heart and the seat of the disease, the distal operation may be performed."

We quote this passage, not with the view of criticising it, but merely because it is a plain statement by an admitted and competent authority of the limited powers of surgery in dealing with this form of disease. The passage is open to criticism, for it ignores the possibility of the treatment of exceptional cases of aneurism of the carotid by direct incision of the sac, as practised in the celebrated instance published by Mr. Syme; and again because it ignores the attempts which have been made to apply pressure to the treatment of carotid aneurism. These attempts, as we learn from a communication made to the Imperial Society of Surgery of Paris, have not been without success, though their number is so small that it is premature to draw any general conclusion from them.

The case laid before the Imperial Society by M. Rouge, of Lausanne, was as follows:—A man, aged sixty-eight, at the time of

[a] The author of this Report, anxious that every contribution to surgical literature should be noticed, will be glad to receive any publications on surgical subjects. If sent to the correspondents of the Journal they will be forwarded.

the report vigorous and in excellent health, was struck on the upper part of the chest by the shaft of a carriage in the year 1854. He suffered but little from the contusion at the time. In 1865 a tumour of the size of a filbert appeared on the right side of the neck. This tumour, being indolent, was neglected for two years, after which time, in 1867, it rapidly increased, and became the seat of acute pain. The nature of the tumour was at this time readily determined by M. Rouge. Still the patient refused all treatment for a year, at the end of which time he became an in-patient of the hospital at Lausanne. The tumour now measured over four and a-half inches in length, and four and three-tenths of an inch in breadth, being situated beneath the sterno-mastoid muscle, and reaching from the criroid cartilage to the mastoid process. The neck was thin, and the artery could be sufficiently isolated below the tumour to admit the consideration of applying distal pressure to it. The pressure was applied during seventeen days, and was maintained for one hundred and thirty hours and a half; at the end of this time the cure, which commenced on the second day was believed to be complete. As a safeguard, pressure was continued for an hour and a half each day for twenty-three days. The cure was after this fully effected. The mode of applying the pressure deserves to be recorded. The direct compression of the carotid against the spine was found, as in previous cases, to be insupportable, in consequence of the pain and cough produced by the pressure on the pneumo-gastric nerve. M. Rouge adopted the method of pressure practised by Gioppi in the treatment of an aneurism of the ophthalmic artery. The thumb was placed beneath the anterior border of the sterno-mastoid muscle, and the three following fingers beneath its external border, and the artery was compressed laterally between the thumb and fingers. The difficulties found in applying the pressure in this manner were—first, that it was not always easy to separate the artery from the nerve, and that occasionally attacks of coughing were induced, when the pressure was accidentally made on both the nerve and the vessel; and, secondly, that the skin became excoriated by the constant application of the pressure on the same part. The pressure produced no cerebral distress beyond slight headache on some days, although the interruption of the arterial circulation caused the sloughing of a vascular warty growth on the right side of the nose.

M. Rouge has collected the cases in which pressure has been applied to the carotid vessel for aneurism of the trunk itself, or of

its branches. These are six in number, and four have terminated successfully. The ophthalmic artery was the seat of disease in three cases, the carotid in three, and in each group there has been one failure to two recoveries. The facts recorded by M. Rouge prove that it is possible to apply pressure with success in aneurismal tumours of the carotid and its branches, and that the statement of Mr. Erichsen can no longer be admitted as correct.

This remark applies similarly to the statement of Mr. Spence, published recently, viz.:—

"As regards the treatment of carotid aneurism, as in that of the iliac arteries, compression is out of the question, and therefore the only plan is to tie the artery."

In opposition to the second statement, that compression in the case of aneurism of the iliac arteries is out of the question, we must refer to the cases published by Heath and Mapother of successful treatment of iliac and ilio-femoral aneurism, with the assistance of the inhalation of chloroform to render the pressure endurable; and to the cases laid before the Medico-Chirurgical Society of London by Hilton and Birkett during the past session. In Mr. Hilton's case there were two aneurisms present, one of which involved the right internal iliac and common femoral, the other the right popliteal artery. A careful examination proved that the right common iliac artery could be so compressed as to command the circulation in both aneurismal tumours, without interference with the circulation in the vessels of the left limb. Digital compression could not, however, be borne, even for a minute. Chloroform was therefore administered, and pressure was made on the vessel for six hours, during which time the patient was kept under the influence of the anesthetic. After eleven days, the first operation having produced no material change in the tumours, two tourniquets were applied, one to the common iliac vessel, the other to the femoral in Scarpa's angle. The pressure was maintained with the assistance of chloroform for six hours. At the end of this time, on the removal of the instrument, the popliteal tumour was hard and free from pulsation; the upper aneurism still pulsated, but feebly however, and was more firm to the touch. The pressure was therefore continued for three and a-half hours more on both vessels. At the end of this period pulsation had ceased in the ilio-femoral and popliteal tumours. The pulse during the entire time was about 72, and anesthesia was maintained to the degree that rendered the

conjunctiva insensible. This second operation was completely successful, and the patient was reported well four months after, except for occasional painful sensations in the limb.

While this case and the previous cases mentioned prove that the treatment may be successful, Mr. Birkett's case shows that the treatment is not free from danger. Mr. Birkett adopted the treatment in consequence of the successful issue in the above case, and because the condition of the patient was eminently unsuited for any cutting operation. The tumour sprang from the left common femoral vessel, and extended upwards into the iliac fossa. Pressure was maintained under chloroform on two occasions, as in Mr. Hilton's case, for eight hours and a-half and nine hours respectively, with an interval of eight days between the operations. After an interval of ten days pressure was applied for ten hours and twenty minutes, but without any improvement in the condition of the tumour. The next day symptoms of pulmonary disease set in, and the patient died after an illness of nine days. Mr. Birkett admits that the fatal illness may have been induced by the chloroform vapour, but he considers that this conclusion rests on very imperfect evidence. He inclines to the opinion that the illness depended more on the depraved constitution of the patient, than on the protracted use of chloroform.

Mr. Spence records a trial of the method of treatment suggested some time since by Sir W. Fergusson, for certain cases of subclavian aneurism, namely, amputation at the shoulder-joint. The modes of treatment commonly applicable to aneurismal tumours, the ligature of the vessel at the cardiac side of the tumour, at the distal side, the direct incision of the tumour or any of the various modes of applying pressure, appeared equally unsuited to the case. The limb below the tumour was in a condition which threatened gangrene. Under these circumstances Mr. Spence considered amputation as proposed by Sir W. Fergusson the most suitable treatment. He hoped by the performance of the operation to effect all that could be done by the distal operation, with the additional advantage of controlling the current through the anastomotic vessels by removing the inducement for the establishment of this current, namely, the upper extremity. He performed the operation, intending to tie the axillary artery high up, but on exposing the vessel he found it dilated (the aneurism being fusiform) to such a degree that a ligature could not be placed with safety on it higher than the commencement of its third stage. This proceeding left a larger

number of vessels springing from the trunk above the ligature than the operator wished or intended to have. The patient recovered the effects of the operation, and for ten days the tumour was scarcely to be felt. When he left the hospital, the aneurism was feeble in its pulsation, and it had diminished to about one-third of its original size. The patient died four years afterwards, the exact cause of death not having been ascertained; the aneurism, however, was believed to have involved the innominate and possibly the aorta. The results of this case would seem to justify the adoption of the treatment in suitable cases, for although it failed to procure the complete cure of the disease, it appears to have arrested its progress for a considerable time, and to have averted the grangrene and its results, imminent at the time of its adoption.

Mr. Spence records the successful treatment of a case of traumatic varicose aneurism of the femoral artery by ligature of the artery above and below the seat of the disease without opening the sac. He maintains that the ordinary treatment of varicose aneurism, entailing the opening of the sac, is inadmissable when the femoral artery is the seat of the disease. In deciding on his operation Mr. Spence obtained but little assistance from the records of surgery. But two cases are, as far as he could discover, recorded, in which operation was performed for the disease in the thigh. In one of these amputation was the operation selected; in the second the femoral vessel was tied, but fatal hemorrhage occurred. Mr. Hodgson suggested the ligature of the femoral, but did not know of a case in which the suggestion had been carried out. Mr. Spence discovered, after the performance of his operation, that Mr. Lawrence had operated by the Hunterian method for varicose aneurism of the thigh unsuccessfully, death being caused by gangrene.

Mr. Lizars is the only authority, according to Mr. Spence, who advocates the mode of operating adopted in this case, as the proper treatment of varicose aneurism in general; and assigned as his reason, the danger of phlebitis if the vein is interfered with. Mr. Spence states that he saw him perform the operation at the bend of the elbow. It would appear from the record of Mr. Spence's case that the risk of phlebitis was not entirely avoided, for the appearance of the limb indicated, after recovery, that venous obstruction probably caused by phlebitis existed.

The mortality attending the three modes of treatment of gunshot injuries of the hip-joint is determined, by the able report issued by

order of the Surgeon-General of the United States Army to be as follows:—Amputation, 183 cases—mortality, 90 per cent.; excision, 85 cases—mortality, 90·6 per cent.; expectant treatment, 122 cases—mortality, 93·4 per cent.

The cases of amputation are collected from all sources, from the first performance of the operation for gunshot injury by Larrey, in 1793, to January 1, 1869, and include 62 cases operated on during the American war. The cases are divided into primary, intermediate, secondary amputations, and re-amputations. Of 79 primary amputations, 1 only is ascertained to be successful. Of 76 intermediate, 6 recovered; of 20 secondary amputations, 7 recovered; and of 8 re-amputations performed for various consequences of amputations of the thigh, 4 recovered. This gloomy list supplies a fact which reverses the statement made in 1861 by MM. Larrey and Legouest, that a successful primary amputation at the hip in military surgery could not be cited. The results of excision, though the number of cases is less than half the number of amputations, are comparatively favourable. Of primary excisions, 39 cases are collected, of which 3 recovered; of 33 intermediate excisions, 3 recovered; and of 13 secondary, 2 recovered.

The author of the report holds, with Guthrie and others, that gunshot wounds involving the hip-joint are almost uniformly fatal when abandoned to the resources of nature by the expectant treatment. He grounds his opinion on the examination of the facts of the cases mentioned above, in which the mortality is stated to be 93·4 per cent; he believes that this examination proves that not one of 49 recoveries recorded in the returns of the American war are really cases of injury of the articulation, with the exception of a single case, in which it appears that early dislocation of the femur on the dorsum of the ilium occurred. In this case the evidence is not conclusive, although the probability is great that it was a true wound of the joint. He opposes to the advocates of the expectant treatment, Perogoff, Demme and Gross; his own opinion in favour of operation, supported by the opinions of the great mass of American surgeons, "who have enjoyed unprecedented opportunities of observation, and who, with a solitary exception, aver that they have never seen a recovery from an undoubted gunshot injury of the hip-joint treated on the expectant plan."

The author of the report lays down the following rules for the determination of cases demanding amputation at the hip-joint or excision:—

Amputation should be performed:—

"1. When the thigh is torn off, or the upper extremity of the femur comminuted, with great laceration of the soft parts, in such proximity to the trunk that amputation in continuity is impracticable. 2. When a fracture of the head, neck, or trochanters of the femur is complicated with a wound of the femoral vessels. 3. When a gunshot fracture involving the hip-joint is complicated by a severe compound fracture of the limb lower down, or by a wound of the knee-joint."

The operation may be admissible:—

"1. When, without fracture, a ball divides the femoral artery and vein near the crural arch; 2. When a gunshot fracture in the trochanteric region is complicated by such extensive longitudinal fissuring as to preclude excision. Secondary amputations and re-amputations at the hip in military surgery should be performed when from caries, or necrosis, or chronic osteomyelitis following gunshot wounds, or amputations in the continuity, the patient's life is in jeopardy. Restricted to the classes of cases above enumerated, coxo-femoral amputation will occasionally save lives that would otherwise inevitably be lost. Primary excisions of the head or upper extremity of the femur should be performed in all uncomplicated cases of gunshot fracture of the head or neck. Intermediate excisions are indicated in similar cases where the diagnosis is not made out till late, and also in cases of gunshot fracture of trochanters with consecutive arthritis. Secondary excisions are demanded by caries of the head of the femur or secondary involvement of the joint, resulting from fractures in the trochanteric region or wounds of the soft parts in immediate vicinity of the joint. Expectant treatment is to be condemned in all cases in which the diagnosis of clinical injury to the articulation can be clearly established."

The author of the report insists on the necessity of establishing, as far as possible, the accurate diagnosis of the extent of the lesion in cases of gunshot injury of the upper part of the thigh bone for three most important reasons—first, that if the joint is involved the case comes within one or other of the conditions implied in the above rules, and therefore operative interference is necessary. Secondly, if the case be one of wound of the upper part of bone, without injury to the joint, the expectant treatment has been found the most successful in the American cases accurately recorded; and thirdly, because incisions necessarily made to establish the diagnosis are desirable, as they tend, even in cases of fracture not involving the joint, to diminish the danger of its becoming secondarily involved by providing a free escape for pus, foreign

bodies, and fragments of bone. It is further advanced in favour of excision, that its performance tends to the relief of suffering, although it may not be successful in saving the life.

The method of excision by a single longitudinal incision, as proposed originally by White, finds most favour with American surgeons; forty of the eighty-five cases recorded were so dealt with, and six of the successful cases were amongst the number. We cannot close our notice of this report, which describes the details of the cases with all the accuracy possible in recording the details of military surgery, without joining in the hope expressed by its author, that "in future wars, through international conventions, such safeguards of field hospitals may be established as to preclude the necessity of their hasty evacuation, and to ensure their inmates undisturbed quiet and appropriate after-treatment." One can with difficulty conceive a patient in a case of excision of the hip-joint surviving an after-treatment of twenty-five miles in an ambulance waggon, followed by seventy miles of railway travelling; yet this has happened; and we can hardly tell how many must have died under similar treatment, for in the American war most patients had to undergo some such ordeal.

Dr. Little holds that true lateral curvature of the spine occurs only as the result of the contraction of the chest, induced by pleurisy; and he proposes to distinguish the ordinary lateral curvature from this deformity by naming the ordinary affection rotatory or rotato-lateral curvature. This term is the translation of the name scoliosis, applied by the Germans to this condition, and has been taken by them from the writings of Galen and Hippocrates. Dr. Little attributes the failure of Dr. Dod's proposal, in the year 1824, of the term rotated spine for lateral curvature, to the fact that orthopedic surgery had not at that time received the impulse which it subsequently derived from Stromeyer's discoveries; he hopes now to establish the term by proving that true lateral curvature is only caused by the contraction of one side of the chest consequent on pleurisy.

Contrasting ordinary or rotatory curvature of the spine with the curvature following the recovery from pleuritis, with effusion and empyema, Dr. Little points out that the curve in the latter case is "more uniform and involves the entire column in one sweep; the depression of the shoulder on the contracted side is relatively greater; the reduction of capacity of the contracted side of the chest applies to the back as well as the front of the chest; the posterior

angles of the ribs on the expanded side and the internal margin and inferior angle of the scapula are less salient, and rotation of the vertebræ on their horizontal axes is entirely absent."

Of the treatment of curvature of the spine resulting from pleuritic effusion and its subsequent absorption, Dr. Little states that any cases in which slight deformity alone exists are cured by the simple resumption of active habits of life. Where, however, the connecting medium between the lung and ribs is dense and firm, and the deformity is marked, special treatment is required and has been successful. Extreme cases, in which the aperture where the empyema was discharged had long remained open, were most rebellious to treatment. The treatment adopted was the sling recommended in cases of ordinary curvature, applied to the sound side in such a manner that the body rested almost suspended in a lateral position. This treatment was applied at first for a short time daily, and gradually increased to several hours in the intervals, the patients being allowed to take unrestrained exercise and amusement. In such cases spinal supports and bandages, as they limit the movements of the trunk, are contra-indicated.

To distinguish apparent slight lateral deformity of the spine, the result of awkward standing, or "trick, from the early stage of real rotatory curvature of the spine, Dr. Little applies the following test:—Let the individual stand erect with the feet side by side, and the knees straight; let the arms be then raised to the utmost over the head, and then let the trunk and head be carried forward and downwards, so as to attempt to touch the feet with the hands without bending of the knees or lateral inclination of the body;—first extension movement of the drill-serjeant—if the suspected curve of the spine disappears in this exercise, "it is due to weakness, temporary relaxation of muscles and ligaments, awkward standing, or trick," and "no persistent deformity and organic departure from the anatomical normal condition of the ligaments and bones of the spine exists." He holds that every lateral curvature in the early stage is curable by:—

1.—Attention to general health and hours of repose.

2.—Gymnastic elongation of the spinal column as in the manner described; invigoration and development of weak muscles by gentle gymnastics, aided by other ordinary appropriate exercises, &c.

3.—Extra use of the arm, leg, and side of body, which were formerly disused during the operation of the exciting cause or

causes of curvature. Without recourse to any kind of spinal couch, spinal support, lying down or extension beds, except in cases where there is a strong hereditary tendency to curvature.

Dr. Little attempts under the head of spinal weakness to describe some definite varieties of disease of the spinal cord, but his ideas on the subject are so vague and so vaguely described that we have failed to comprehend them.

Dr. Bauer[a] dissents from the principles of treatment of talipes recommended by Barwell, Prince, and others, namely, extension of the contracted muscles and tendons, instead of the subcutaneous tenotomy and myotomy on the following grounds:—The claimed efficiency of extension rests on the pathological supposition—first, that the contracted muscles are in a state of continuous spasm; or, second, that the muscles are merely shortened from the physiological fact of paralysis of their respective antagonists. To the first of these suppositions he objects that even in cases where the cause of the contraction is spasm, caused by irritation of the nerves of the effected muscles, the extension treatment fails to overcome the deformity, and to the second that where paralysis resulting from the accidental division of the nerve of a group of muscles such as the peroneal exists, the deformity produced is not of the kind that is met with in ordinary talipes, and can be readily overcome by simple means.

Dr. Bauer discusses at some length the various theories put forward to account for the production of ordinary lateral curvature of the spine. He rejects, as untenable, the assertion of Lorinser recently put forward, that the active cause of the deformity is osteomyelitis of the bodies, a theory which is unsupported by any fact, save that in old confirmed cases of curvature, the bodies are connected to each other in part by new bone. He holds that Lorinser's pathological investigations have been made only on cases where the deformity was clearly due to rachitis. The doctrine of Delpech of disturbed muscular antagonism, he maintains, has been set aside as erroneous by the investigations of Werner; the similar views of Guéren and Stromeyr have already been proved by Malgaigne to be unable to bear the test of their application to practice, for his cases treated on their principle by tenotomy were found to be in no way remedied by the operation; on the contrary, they were in many cases aggravated by it. Dr. Bauer holds that a consideration of the facts proves that "scoliosis

[a] Bauer, Lectures on Orthopedic Surgery. Second edition. New York, 1868.

bears a close connexion to a certain age, to the female sex, its evolution, and a certain general condition of the system and spine." He has derived this explanation of the pathology of lateral curvature from Dr. Buchring, of Berlin, who attributed the source of curvature to a low state of homatoris at the period referred to.

We fail to find in this explanation any definite pathological explanation of the source of lateral curvature.

Dr. Bauer records the occurrence of detachment of the lower epyphysis of the femur in four cases, in his practice during attempts to straighten a deformed knee-joint. He thinks, however, that this accident is not of any great consequence, as if it be promptly recognized and attended to, union is perfect in six or eight weeks, and the attempt at extension can be renewed without hazard. He states, however, that in one of the four cases, the accident led to amputation of the thigh.

REPORT ON MATERIA MEDICA AND THERAPEUTICS,[a]

By Walter G. Smith, M.B, Dub., Fellow and Censor K. & Q.C.P.I.; Senior Demonstrator of Anatomy in the School of Physic; Assistant Physician to the Adelaide Hospital.

MATERIA MEDICA AND GENERAL THERAPEUTICS.

1. *Syrup of Iodide of Iron.*—Mr. Carteighe concludes from his experiments that this syrup may be preserved for a long period by exposure in white glass bottles to direct sunlight. Dr. Redwood's experience is that, if properly made and preserved, it could be kept without change for several years, and he suggests that the causes of failure in most cases lie in the sugar being inferior or insufficient in quantity.—*Pharm. Jour.*, Sept., Dec., 1869. (See last Report, Art. 1.) I have a specimen in my possession which was made in the year 1849, and is still perfectly clear and free from sediment. It was made without heat, and with the syrup of the Dub. Pharm. 1850.

[a] The author of this Report, anxious that no contribution to the subjects of Materia Medica and Therapeutics should remain unnoticed, will be glad to receive any publications which treat of them. If sent to the correspondents of the Journal (see page iv.) they will be forwarded.

2. *Substitute for Dover's Powder.*—Dr. A. Chapin, dissatisfied with the ordinary Dover's Powder on account of its nauseous and offensive taste, has for the last twenty-five years used the following substitute with great satisfaction:—℞ Pulv. opii., 3i.; pulv. ipecac., 3ss.; pulv. camph., 3ii.; sacchari, 3iv. Mix thoroughly.—*Med. and Surg. Rep.*, July, 1869. A preferable mode of combining opium and hippo is to give ext. opii. and pulv. ipecac. in pill. The nauseous taste of the drugs is thus concealed.

3. *Toxic Properties of Arnica.*—It appears that this drug is not so innocuous as is, perhaps, sometimes thought, for a case of poisoning by tincture of arnica was reported in the *Annuaire Pharm.* for 1866, and another, from drinking an infusion of arnica leaves, has been recently published in the *Jour. de l'Acad. de Turin.* The symptoms came on in half an hour, and were those of an irritant poison—vomiting, purging, abdominal pain, prostration, &c. Both cases recovered under the use of opium.—*Journ. de Chim. Méd.*, Sept., 1869.

4. *Chlorodyne.*—The composition of this secret remedy has been the subject of much controversy, and several formulæ purporting to be imitations of, or substitutes for it, have been published. There can be little or no doubt that the *essential* ingredients are opium (or morphia), chloroform, hydrocyanic acid, and, perhaps, Indian hemp. "A Provincial" is convinced that chlorodyne is "only a roundabout and disguised mode of administering morphia, slightly modified by the chloroform;" but Dr. Dowse suggests with plausibility, from observation of a case of poisoning by Collis Browne's chlorodyne, and from a consideration of the physiological action of the drugs in question, that belladonna (or atropia) plays an important part in this compound.—*Pharm. Journ.*, Sept. and Nov., 1869.

5. *On the Influence of Chloroform in Promoting Cutaneous Absorption.*—The practical importance of such an inquiry in reference to the application of external remedies is so evident that the conclusions at which Dr. Aug. Waller arrives are of great interest. As the result of his experiments he finds that—(1.) Chloroformic solutions (of atropia, aconite, strychnia, morphia, &c.) are quickly absorbed by the skin, and produce local and general results, according to the substance employed. (2.) Alco-

holic and aqueous solutions are either not at all or very slowly absorbed. (3.) Chloroform easily traverses the dead skin by diffusion. (4.) Alcohol does not traverse the skin, but produces an endosmotic current with water. (5.) Skin exposed to chloroform in a state of liquid or vapour absorbs a considerable quantity of it. (6.) On traversing the septal skin of the endosmometer, chloroform carries with it a certain amount of any alkaloid dissolved in it. (7.) These observations sufficiently explain the rapidity of cutaneous absorption during life, without our having recourse to any problematic influence of sebaceous matter on the surface of the skin.

Dr. Waller states also that the addition of astringents to a solution of atropia in chloroform has no influence in retarding the absorption of the atropia.—*Practitioner*, Dec., 1869.

6. *Combined Action of Chloroform and Morphia.*—M. Claude Bernard has lately investigated this point. Morphia produces sleep, but at the same time it brings on a peculiar condition of excitement. It does not cause anæsthesia. If chloroform be inhaled by an animal previously narcotized by the alkaloid, only an extremely small dose is necessary to produce anæsthesia. On the other hand, the injection of muriate of morphia in an animal which has been set to sleep by chloroform, and is beginning to awake, restores insensibility.—*Lancet*, Dec. 4th, 1869.

M. Uterhart has in several cases successfully employed the subcutaneous injection of morphia as a safe means of prolonging the effects of chloroform. He injects half a grain of morphia six to ten minutes before commencing the inhalation of chloroform.—*Centralblatt*, No. 36.

Mr. Wheelhouse tells us (*Brit. Med. Journ.*, 25th Dec., 1869, p. 690), that in Leeds, "besides giving our patients about to receive chloroform a dose of brandy before coming into the operating room, we often, especially if they are very nervous about it, and sometimes even when taking it if they get very excited, administer one-sixth of a grain of morphia by hypodermic injection, and with most marked advantage." He does not specify particularly what the effects of the injection were, and it would be of great interest if hospital surgeons would test this point carefully. Liebreich's suggestion also, of trying chloral in combination with opium or morphia, is well worth attention.

7. *Opium Alkaloids.*—M. Woldemar Baxt divides the group of

opium alkaloids into those which are narcotic and those which are tetanically convulsive:—

Narcotic	Tetanisers
In the order of their activity.	
Papaverine	Thebaia
Morphia	Porphyroxia
Narceia	Narcotine
Codeia	Codeia

Claude Bernard divides them into three classes, arranged in the order of intensity of action:—

1°. *Excitants*—Codeia, morphia, narceia. 2°. *Soporifics*[a]—Narceia, morphia, codeia. 3°. *Convulsants*—Thebaia, narcotine, papaverine. Thebaia is the most poisonous.—*Arch. f. Anat. u. Phys., Gaz. Hebdom.*

8. *Sinapisms.*—The activity of mustard, as is well known, depends chiefly on its essential oil, which is a *product* of a kind of fermentation between two substances, in which an albuminous matter, myrosyne, plays the part of ferment, and acts on a peculiar fermentescible compound, myronate of potassium. This sinapisic fermentation can only occur under the ordinary conditions of fermentation, *i.e.*, in the presence of water, and at a temperature above zero and below 75°C. (167°F.) Hence to obtain the full effect from a mustard poultice it is necessary to be mindful of the fact that the production of essential oil diminishes between 50° and 75°C., and ceases altogether above the latter point, and consequently that water which is boiling, or even too hot for the hand to bear, is unsuitable for the preparation of sinapisms or mustard pediluvia.

Further, as in the case of other fermentations, certain substances are opposed, more or less, to the formation of the volatile oil, *e.g.*, alcohol, metallic salts, and acids. Vinegar is sometimes added to mustard poultices with the intention of increasing their efficiency, but wrongly, for MM. Trousseau and Blanc have ascertained that it diminishes their rubefacient power.—*Journ. des. Conn. Méd.*, Avril 10th, 1869.

9. *On the Dangers of Blistering, especially in Pneumonia.*—Although Dr. Dauvergne takes as his text only the employment

[a] Although Claude Bernard does not accord any soporific power to papaverine, MM. Liederdorf et Bresslauer, Drs. Baxt and Stark agree in considering it as one of the best and least hurtful of the soporifics.—*Rep. de Pharm.*, Oct., 1869.

of blisters in pneumonia, he enters into the discussion of the physiological and therapeutical effects of blisters, and of the nature of the so-called principle of revulsion. His arguments, which apply, of course, to the consideration of other affections besides pneumonia, are deserving of serious attention. Since the year 1835 he has renounced the treatment of pneumonia by blisters for the following reasons, on which he has perfectly satisfied himself:—

1°. Blisters in no way diminish the duration of the disease. 2°. They increase the febrile excitement. 3°. They are a real misery to the patient. 4°. Each dressing involves danger, because it is necessary to uncover the chest. 5°. Finally, a fresh inflammation developed near an inflamed part is more likely to aggravate than to divert the disease from the latter.[a]

The stethoscope, moreover, reveals no improvement in the physical signs after vesication, and the fever itself is actually increased. In support of his views he cites Louis, Andral, Trousseau, Coste, Forget, Malgaigne, &c., men whose opinions must always be of the greatest weight, and adduces various cases showing the serious effects resulting from the abuse of blisters, which most practitioners will be able to supplement from their own experience. He endeavours to show, and with reason, that the celebrated aphorism of Hippocrates—*Duobus doloribus simul obortis vehementior obscurat alterum*—really lends no support to the revulsive theory as applied to inflammation (*i.e.*, blisters), particularly if we excite inflammation near to disease. And he scouts the idea of applying blisters to the head in cerebral inflammation; because, in the first place, he has never seen any but disastrous results from such treatment, and, secondly, he considers it to be highly illogical to produce an ulceration—an inflammation—on the scalp, when there is already an inflammation of the brain, and perhaps of the membranes, remarking that in erysipelatous inflammation of the scalp it is dreaded lest the cutaneous inflammation should extend to the membranes or to the brain itself. Dr. Dauvergne allows the utility of mild derivation, *e.g.*, sinapisms and dry-cupping, in the commencement or congestive stage of inflammation, or, still better, at the end of acute diseases

[a] The benefit or injury to be derived from the use of blisters depends so much on the stage of the disease at which they are applied, that these condemnatory clauses must be received with limitations.

when the organic machinery is failing, when external heat diminishes and abandons the extremities, and when the sensibility is lowered. The influence of blisters in modifying pain and the changes of sensibility which they produce in the neuroses, such as sciatica, intercostal neuralgia, and some varieties of tic-douloureux, are recognized, and he admits also the value of blisters in certain serous effusions, as in latent pleurisy, provided there be *no fever* present, in encysted goitres with precaution, in indolent hydrarthroses, some recent hydroceles, and possibly in congenital hydrocephalus. In such instances blisters act, not by revulsion, but by simple osmosis. Yet, in all these cases, he prefers other means, *e.g.*, iodine or compression. Lastly, he instances their use in surgical affections, such as erysipelas and diffused phlegmon, where they arrest the disease by their genuine inflammatory action, drawing into a limited area the fluids of one and the same set of capillaries. They thus precipitate the resolution of the inflammation by provoking a premature suppuration at a part already disposed to this process. Even in these cases, however, he is of opinion that blisters are not indispensable, and may be advantageously replaced by other means. Dr. Dauvergne concludes his thoughtful and outspoken review of blisters by asking "What remains of their benefits? But little, and, on the contrary, many inconveniences. The surgeons alone may sometimes rehabilitate them, and in fact it is they who have so widely extended their use. Their habituation to seeing the disease, to be always doing something, must go for much in their retention of suppuration as a remedy; but that is not rational therapeutic physiology."—Translated by Dr. Anstie in *Practitioner*, Oct., 1869. (See last Report, August, 1869, Art. *Counter-irritation*, p. 192.)

10. *Influence of Alcohol on the Temperature of the Body.*—According to Professor Binz, the effect of very small quantities of alcohol on men and dogs, is invariably to produce a gradual fall in the temperature of several tenths of a degree C.

When large or fatal doses were given to dogs or rabbits a constant fall in temperature also occurred, amounting to between 3° and 5°. Alcohol decidedly diminished the elevated temperature of an artificially produced febrile condition, and the so-called fever of digestion either did not recur at all, or only to a very slight degree, when alcohol was taken. But, on the other hand, in opposition to its antipyretic effects, the number and strength of the heart's

contractions were augmented in all the experiments. Dr. Binz attributes the refrigerating power of alcohol to an influence exercised directly on the oxidizing processes that occur in the tissues of the body.—(*Practitioner*, Sept., 1869.)

11. *Chemistry and Physiology.*—Dr. A. Rabuteau believes that he is justified by his investigations in propounding as a general law that "the metals are more active physiologically according as their atomic weights are more elevated, or, what is the same thing, as their specific heat is lower." For example, sodium (Na=23) is nearly inert; potassium (K=39·1) is active in large doses; and thallium (Tl=204) is a dangerous poison, nearly as poisonous indeed as lead (Pb=207). The only exceptions that appear to exist to this *atomic law*, are in the case of rubidium tungsten and molybdenum, which are not active when combined with inactive metals.

As respects the metalloids, the diatomic metalloids are entirely conformable to this law, but the monatomic elements, curiously are governed by a law which is the reverse of this, as Bouchardat and S. Cooper had already shown, who observed that chlorine (Cl=35·5) is more active than bromine (Br=80), and the latter more so than iodine (I=127). Dr. Rabuteau now shows that fluorine (F=19) conducts itself in accordance with this latter law, for the fluorides are the most active among the compounds of the monatomic metalloids. Dr. Rabuteau proves also that the organism acts as a reducing agent, converting bromates into bromides, iodates into iodides, and sulphates into sulphites and hyposulphites.—(*Journ. of Anat. and Phys.*, Nov., 1869; quoted from *Journ. de l'Anat. et de la Phys.*, No. 2, 1869. In connexion with this subject, Dr. Broadbent's ingenious and suggestive "attempt to apply chemical principles in explanation of the action of remedies and poisons," will also prove of interest.—(*Phil. Mag.*, Nov., 1868; quoted in *Brit. Med. Jour.*, Nov. 13th, 1869.)

12. *Effects of Remedies upon the Cerebral Functions.*—Dr. J. Nicholls has known aphasia to be produced by the use of two drugs, viz.:—stramonium and cannabis indica.

1°. *Stramonium.*—An elderly gentleman was in the habit of smoking stramonium very freely, mixed with a small quantity of tobacco, for the relief of asthma. After several pipefuls "he would begin to call things by wrong names—*e.g.*, his boots, logs of wood; his

bed-room, the stable, &c., and this without knowing it; and it is remarkable that he almost always miscalled the same things. He was a very small drinker, and always remembered the subjects of his conversation, but not his language." After a sound sleep he would regain the power of speaking intelligibly. Dr. Paget Blake met with a case of an old gentleman who was poisoned by accidentally taking 3iss. of Tr. stramonii. When he began to recover from the immediate depressing effects of the poison, he could not articulate a syllable, and made ineffectual attempts to speak. " He could not speak at all intelligibly till the end of the next day, and then continually misplaced words, calling his head his foot, his arm his leg, and misnaming the things he required, though ludicrously unconscious of his perpetual misnomers. It was several days ere he could converse without calling something by a wrong name."—(*St. George's Hosp. Rep.*, 1868, p. 159).

2°. *Cannabis Indica.*—A young man, aged twenty-four, affected with paraplegia, and suffering from excruciating pain in the head, could obtain but little relief from the use of the ordinary anodynes. On trial it was found that cannabis indica gave relief, but large doses were necessary. After a large dose, *i.e.*, 3ii. of Tr. cann. ind. with a little hyoscyamus, he was free from pain, " but in talking he misnamed almost everything—called his head his foot, &c. And this was always the case; but he was and is now conscious that when he has taken the drug he cannot express his ideas correctly, whereas in the stramonium case the patient did not remember that he had miscalled things." Dr. Ogle had previously noticed a case in which words were wrongly used by a patient whilst under the influence of opium, and Dr. Leuen, a French physiologist, states that quinia, in addition to other effects, may produce aphasia.[b]—*Lancet*, Aug. 21st, 1869.

3°. *Bromide of Potassium.*—Two curious cases are recorded in the *British Medical Journal.* A patient subject to " le petit mal.," began with 20 grs. daily of the bromide, and gradually increased it up to 70 or 80 grs. *per diem.* Profound, yet disturbed sleep was caused, but more especially, enfeeblement of mental power. He could scarcely balance his accounts and used to substitute one word for another, " in a most provoking and yet ludicrous way." Though the right idea was present to his mind, he found great difficulty in

[b] Dr. W. Ogle has collected a number of cases which show that loss of speech is no very uncommon consequence of the bite of a venomous snake.—(*St George's Hosp. Rep.*, Vol. iii., 1868, p. 167).

hitting on the right words. When he ceased taking the drug for a time all the unpleasant symptoms faded away; the slow uncertain speech, the heaviness of eyes, the stoop in his figure, &c. He now never exceeds 40 grs. a day, often takes only 20 grs., and if he finds the slightest tendency towards talking "Mrs. Malaprop" English he reduces the dose at once.—(Oct. 16, 1869).

Dr. Anstie has also called attention to the occasional temporary loss of memory caused by bromide of potassium.—(*Pract.*, Sept., 1869.) A remarkable case is narrated by Dr. W. A. Hammond (*Quart. Journ. of Psycholog. Med.*, Jan., 1869), in which 30 grs. of the bromide three times a day, produced, in a man suffering from paralysis agitans, profound melancholy, with delusions, contraction of pupils, drowsiness and failure of memory. The symptoms of the disease were entirely arrested, and although he continued the medicine, most of its objectionable effects passed off after a few days.—*Brit. Med. Journ.*, Jan. 1st, 1870.

Mr. Lockhart Clarke has recently witnessed still more remarkable effects from only two ʒss. doses of this medicine. A man, who was not epileptic, took one dose at 11 a.m., and the other at 5.30 p.m. the same day. At 12 o'clock that evening, he described his sensations thus:—"I feel as if everything that I feel, see, or think, had been experienced before, as in a dream. This delusion is so strong, that I could swear I dreamt it all." Next morning when Mr. Clarke saw him, he was perfectly rational, but in such a confused state that he was wholly unfit for any kind of occupation, and when he spoke to his sister began to cry. He gradually recovered the use of his senses, and in the afternoon about 5 p.m., suddenly remembered having come home on an omnibus the night before, and before going to bed he was quite himself again.—*Brit. Med. Journ.*, Oct. 30th, 1869.

In connexion with these cases, it may be remarked that Dr. Anstie speaks of the occasional supervention of symptoms of *temporary* spinal paralysis, and that Dr. Porter (America), has seen 4 grs. of bromide of potassium repeatedly produce in a sensitive lady, numbness in the extremities, and shooting burning pains throughout the whole system, both gradually passing off without other evil consequences.—*Practitioner*, Nov., 1869, from *Hay's American Journal.*

13. *Toxic Action of Quinia.*—Cinchonia has been credited by Dr. Hinkle with occasionally causing an erythematous blush on the

skin of the face and upper extremities in young children, but such an effect is not commonly ascribed to quinia. Lately, however, three interesting cases have been recorded, in each of which the administration of a small quantity of quinia was followed by an erythematous rash resembling the eruption of scarlatina. Mr. Garraway's case occurred in the person of a lady, aged forty, in previous good health. She had taken about 1 gr. of quinia (sulphate) in a glass of wine, and she was suddenly seized with a feeling of uneasiness in the præcardia, attended with œdema of face and limbs, and "an unusual erythematous rash." No unwonted article of food had been taken, and after three or four days the œdema and rash subsided, and the skin of the face, hands and feet peeled off. A tonic quinia mixture was subsequently unreflectingly ordered, and two hours after taking the first dose (2 grs.), all the former symptoms recurred.

Not long after this case occurred, Mr. W. B. Hemming prescribed for an elderly lady, who was recovering from a very severe accident, a mixture containing 1 gr. of quinia in each dose. Next morning, when summoned to see her, he found that she had passed a sleepless night, and observed a vivid red rash all over the body, attended with intolerable and incessant itching. There was slight puffiness of the face, but no œdema elsewhere; a white tongue, and slight uneasiness in the præcordia. The patient at once said "I know this is the quinia, as it occurred twice before, and more severely when I took it in France some years ago." No error in diet could be discovered. The rash and irritation were persistent for several days, and then slowly and gradually subsided, followed by universal exfoliation of the cuticle. There were no other symptoms of constitutional disturbance, and the patient had taken only two doses of the mixture.

Dr. J. C. Thorowgood recalls to mind the case of a young woman, among the out-patients at the Victoria Park Hospital, who, at her second visit, told him that he had been giving her quinia. This was the case, and she knew it to be so from a rash having come out all over her body after two or three doses had been taken. The eruption reminded Dr. Thorowgood of measles, and the girl told him that on four or five occasions, when quinia had been prescribed, a similar rash had invariably appeared.—*Brit. Med. Journ.*, Oct. 9th, Nov., 13th, Dec. 11th, 1869.

Dr. R. Lightfoot observed in 1868, an itchy papular and erythematous rash come on in half an hour after taking one dose of

citrate of iron and quinia (containing 1½ gr. citrate of quinia). This occurred in a lady, aged forty-six, of a nervous irritable temperament. The eruption disappeared in three hours.—(*Brit. Med. Journ.*, Jan. 8th, 1870.) Hæmaturia is also mentioned as occasionally following the administration of sulphate of quinia.—(*New Orleans Journ. of Med.*) A boy, aged thirteen, was affected with hæmorrhage from the urinary organs each time that quinia was ordered, and in the case of a little girl, aged seven, the administration of quinia was invariably followed by urinary hæmorrhage.—*Brit. Med. Journ.*, Jan. 1st, 1870.

SPECIAL THERAPEUTICS.

14. *Aconite, Poisoning by—Hypodermic Injection of Ammonia.*—Mr. B. Wills Richardson taking the hint from Professor Halford's experiments in reference to ammonia as an antidote to snake poisoning[a] was led to employ it in this case. A young lady, aged 25, swallowed by mistake, immediately after breakfast, two tablespoonfuls of an aconite lotion, equal to about ℥iss. of tinct. aconite at 11 o'clock, a.m. About 12.30 she became alarmingly ill with the usual symptoms of aconite poisoning, severe vomiting set in and the pulse could scarcely be felt. Stimulants were at once rejected by the stomach. Mr. Richardson saw her at 6.45 p.m.; at 6.50 she was pulseless, and extremities deathly cold, pupils dilated, intellect unimpaired. At 7 p.m. he injected ℥ss. of liquor ammoniæ under the skin of the arm. The injection was repeated three times at short intervals, and after the fourth injection the radial pulse reappeared, the vomiting had almost ceased, and she began to improve progressively. The tingling of the lower lip continued for sixteen days; an eschar followed one of the injections. Mr. Richardson suggests the trial of ammonia subcutaneously in chloroform poisoning and in hydrophobia.—*Med. Times and Gazette*, Dec. 18th, 1869.

15. *Aneurism.*—1°. *Hypodermic Injection of Ergot.*—M. Langenbeck from a consideration of the action of ergot in causing contraction of smooth muscular fibre was led to employ it in aneurism. He gives two cases, one of subclavian aneurism, the other, of radial aneurism in which its use was attended with most

[a] Dr. Fayrer, of Calcutta, has performed some experiments which throw the gravest doubts on the asserted efficacy of this antidote.—(See Lancet, March 20th, 1869).

decided success, the effects being very marked. In the case of the radial aneurism, which was of twenty years' standing, sacciform, and about the size of a filbert, a single injection of 0.15 gramme of the aqueous extract of ergot produced a complete and rapid cure. In the former case he injected from 0.03 gramme to 0.18 gramme of the extract at a time over the aneurismal sac. The solution is made as follows:—aqueous extract of ergot (Bonjean) 2 parts, rectified spirit 7 parts, glycerine 7 parts.—*Berlin Klin Woch.*, vi. 12, 1869. Translated in *Gaz. Hebdom*, July 16th, 1869.

2°. *Iodide of Potassium.*—Dr. G. W. Balfour in some "Observations on the Treatment of Aneurism," reports eleven cases, chiefly thoracic aneurisms, treated by iodide of potassium. Not only relief to symptoms but positive improvement was obtained in every case submitted to this treatment for a sufficient length of time; and Dr. Balfour thinks that there are many facts which tend to prove that iodide of potassium is not only curative in internal aneurisms already developed, but that it also acts remedially and prophylactically in the aneurismal diathesis.—*Edinb. Med. Journ.*, July, 1869, see also for July, 1868.

16. *Atropia Collyrium, Poisoning by.*—M. Richet records an instance of an old man, about eighty years of age, who suffered in a most decided manner from the instillation of atropia into the eye. He had been operated on for cataract two months previously, and as false membranes were formed in the pupillary field, a solution of atropia (1 in 100) was dropped in morning and evening in order to expand the iris. After this practice had been continued for about a week, the patient was one day seized with shivering and subsequently with delirium, which came on every day at the same hour. This did not, however, prevent his answering questions, nor did it interfere with his appetite, though he was so violent at night that it was necessary to employ a strait waistcoat. When the atropia instillations were withheld he recovered perfectly in a few hours. He could not have imbibed more than two milligrammes of atropia. M. Laurenzo (Brazil) relates an analogous case in the *Gaz. des Hopit.*, Oct. 21st, 1869, and Dr. Nieberg two years ago (*Bull. de Thér.*, 1867) observed a case in which a strong solution of atropia had accidentally been too rapidly dropped into the eye and acute symptoms of poisoning were rapidly manifested.

Liebreich has shown that the symptoms of poisoning consequent on the instillation of atropia depend mainly upon the quantity which

makes its way through the lachrymal passages into the nose, pharynx, and stomach. When these lachrymal passages are obliterated by disease or otherwise, a strong solution may be applied to the eye for any length of time without fear of any bad consequences. In order to prevent, as far as possible, absorption by this channel, Liebreich recommends that the patient should incline his head forward, should gargle frequently, and blow his nose, and should press one of his fingers against the inner angle of the eye, so that the lower lachrymal point should be drawn down. A small wire apparatus is described and figured (*Dict. de Méd., Atropine*) which effectually prevents the escape of the solution.—*Med. Times and Gazette*, 1869.

17. *Bromide of Potassium.*—1°. *Adulterations.*—It could scarcely be expected that this "marvellous salt" which, we are told, is now manufactured in England alone, for medicinal use, at the rate of a ton a week, would escape serious adulteration. M. Marais has found it to be adulterated with sulphate of potassium, as much as 5 per cent.; with carbonate of potassium, as high as 10 per cent., and with chloride of potassium in greater or less quantity.—*Journ. de Phar. et de Chim.*, Aout, 1869.

M. Adrian (*Bull. Gén. de Thér.*, Juillet, 1869) examined the purity of commercial samples of bromide of potassium sold in Paris. He found chloride of potassium in all the specimens, and also notable amounts of caustic potash, carbonate of potassium, and sulphate of potassium; traces of bromate of potassium, and iodide of potassium in small quantities in two samples out of ten. The quantity of pure bromide varied from 62 to 91 per cent. Some of these impurities are no doubt due to the French mode of preparing the salt.—*Practitioner*, August, 1869.

Dr. Garrod found, some years ago, that bromide of potassium often contained iodide of potassium, sometimes in such quantity as to induce iodism where the salt was given in large doses.

2°. *Best Mode of Administering Bromide of Potassium.*—M. Maréchal, observing, in passing, that this salt is frequently of very bad quality, containing chloride or iodide of potassium, finds that the only preparation which is really well borne by patients is the syrup of bromide of potassium (free from iodide) made with the syrup of bitter orange peel, which contains two grammes of bromide in a spoonful, (*sic.*)—*Gaz. des. Hop.*, Mars. 4, 1869.

3°. *Physiological Action.*—Dr. R. Amory sums up his investi-

gations in the following propositions:—(1.) Bromide of potassium is easily absorbed by the mucous membranes. (2.) This drug is *readily* absorbed by the skin, provided the solution is below 75° F. If the temperature is above 96° F. it is not absorbed. (3.) The elimination is conducted by the skin and kidneys. (4.) In therapeutical doses the bromide is not eliminated by the intestines or lungs. According to M. Rabuteau the bromides are eliminated by the salivary glands, kidneys, nasal and bronchial mucous, fæces, and in the perspiratory fluid (*Gaz. Hebd.*, Mars. 19, 1869). (5.) Bromide of potassium passes out of the system without decomposition. (6.) The effects of the drug are produced by its direct action on the blood vessels, or the vaso-motor system which controls the contraction of these vessels. This explanation may account for all the physiological and therapeutical conditions brought about by the exhibition of this drug. (7.) There is probably no different or opposing action in proportion to the dose administered, which cannot be said of all drugs. (8.) Its action upon the general nervous system is secondary and dependent upon that of the vaso-motor nerves. (9.) Bromide of ammonium in *almost* every respect has the same action as bromide of potassium.

Dr. Amory protests against the use of bromide of potassium in epilepsy when dependent on cerebral or spinal anæmia, in which case it would aggravate the disease.

Compare with this Dr. Purser's valuable paper (*Dub. Quart. Journ.*, May, 1869) on the Physiological Action of Bromide of Potassium. Dr. Purser took great pains to satisfy himself that the first and chief action of bromide of potassium on the nerve centres is the abolition of reflex movement, and "to this power of lowering the reflex excitability of the nerve centres is probably due the chief efficacy of bromide of potassium in those diseases for which it is successfully prescribed."

4°. *Therapeutic Applications.*—(See also *Art.* 20).—Dr. Hargis cured a severe case of *priapism* consequent on, and immediately following, the act of copulation by a few 15 grain doses of the bromide. Another attack, under similar circumstances, two months after, was similarly cured, although the patient had been affected for fourteen hours, and had undergone other treatment without relief.—*New Orleans Journ.*, July, 1869.

M. Moutard has administered this salt with the happiest results, as a sedative and hypnotic to young children, especially in cases of *wakefulness.* It should not be given to children affected with diarrhœa.—*Jour. des Connaiss. Méd.*, Dec., 1868.

5°. *Cutaneous Eruptions produced by.*—M. Voisin has observed several forms of eruption to follow the administration of this medicine. He describes them as acne; large, indolent, and painful pustules; an eruption resembling urticaria or erythema nodosum; boils; and eczema. The first two are especially characteristic.—*Brit. Med. Journ.*, Feb. 27, 1869.

Dr. Cholmeley lately communicated to the Clinical Society of London a severe case of confluent acne of the face and legs excited by bromide of potassium.—*Brit. Med. Journ.*, Dec. 11th, 1869.

18. *Calabar Bean.*—Mr. Wharton Jones has lately announced an observation which, if confirmed, will be of great practical importance, viz., that while atropia causes contraction of the *arteries*, calabar bean contracts the *veins*. (*Practitioner*, September, 1869). Dr. Eben. Watson contributes (*Practitioner*, Sept., 1869), six additional cases of acute traumatic *tetanus* treated by the bean. Five died and one recovered. Dr. Watson thinks that these results, contrasting so unfavourably with his first experiences, may be in part explained by the varying quality of the drug, for there is not yet any regular supply of the beans, and he finds that some of the extracts sold are different from others, physically and physiologically. He administered the crude powder when the patient could swallow it, 10 to 12 grs. every hour, or $\frac{1}{6}$ gr. to $\frac{1}{3}$ gr. of the extract hypodermically, or 1 gr. extract by the mouth, or 20 to 30 m. of the tincture (5 m. = $\frac{1}{12}$ gr. extract). A table of 18 cases is given, 10 from his own practice and the rest from various sources, showing 10 recoveries and 8 deaths. Dr. Watson points out that the physiological effects of the drug form the best warrant for its employment in tetanus, and urges that while we had no reason to look even for benefit from many of the remedies formerly used in cases of tetanus, we have reason from physiology to expect relief in that disease even where we cannot hope for cure from the calabar bean, and this has been marked in his experience of it.

Dr. Monti tried this treatment in four cases of trismus neonatorum, and one of traumatic origin in a boy aged four. The traumatic case and two of the infants recovered. The remedy was mostly administered subcutaneously. $\frac{1}{30}$ to $\frac{1}{6}$ gr. of extract for an infant (*Jahrb. f. Kinderheilk.* Mai). Dr. C. Sherrard was unsuccessful in a case of trismus on an infant four days old, notwithstanding the early administration of the drug internally (*New York Med. Journ.*, Nov., 1869). Dr. Haining records a case

of traumatic tetanus in the *Lancet*, which exemplifies the beneficial effects of this drug when used very freely. As much as 1 to 4 grs. of the extract were given in little more than twenty-four hours; the largest individual dose being 6 grs. (Was the extract of good quality?—the dose is enormous).—See previous Report, Art. XVI., *Dub. Quart. Journ.*, Aug., 1869.

19. *Carbolic Acid.*—In Mr. Edgar Browne's opinion, who has written a useful paper on the merits of carbolic acid in the treatment of skin diseases, as compared with other and older remedies, carbolic acid may, "on the whole, be considered a versatile jack-of-all-trades, doing many things fairly well, but in all (except as a caustic) inferior to some master remedy either in efficacy or convenience." Its action in the tineæ and other diseases seems to be in no way specific, but to depend upon its power of coagulating albumen, stimulating the skin, and excluding the atmosphere. With trouble and care it may be made to do all that remedies possessing these qualities may be made to do—no more. It is in no sense of the word magical in its effects, nor can it be regarded as an important novelty in the therapeutics of the skin as it can in some other departments. In the parasitic diseases Mr. Browne still holds to corrosive sublimate; but he finds carbolic acid to act remarkably well in sweating feet, and recommends it to be used as a lotion, gradually increasing the strength till it stings.—*Practitioner*, Dec., 1869.

Dr. Sansom has proposed the use of the *sulpho-carbolates* for internal administration, as retaining the antiseptic properties of the acid while devoid of its caustic and irritant action. They are all soluble in water; but the most suitable compound is sulpho-carbolate of sodium. Mr. J. Wood has for the last year used sulpho-carbolate of zinc as an external application to wounds and suppurating abscesses, and after operations and in gonorrhœa and venereal sores has had better results from it than from any other means.—*Brit. Med. Journ.*, March 13th and 20th, 1869; *Med. Chir. Trans.*, LII.

20. *Experience of various Drugs.*—Dr. M'Gregor records, in the *Edinb. Med. Journ.* for October, 1869, his actual experience in hospital practice of some of the recently-vaunted remedies. There was a daily average of 350 patients under medical treatment in the institution, and of them 150 were insane. Some of his observations

are of especial interest to the physician. 1°. *Bromide of Potassium.*—He has never yet been able *to cure* a single case of epilepsy by this drug, yet he has found it a most valuable agent in its mitigation and relief. The disease always reappeared on its withdrawal or soon after. The usual dose was ℈i. three times a day to begin with. In some bad cases he gave ℈i. every two hours. The drug undoubtedly had the power of holding the disease in abeyance. He never found the salts of zinc or silver of the slightest benefit in epilepsy.

2°. *Blistering in Acute Rheumatism.*—He speaks most highly of the advantage of applying fly-blisters in every case of acute articular rheumatism. Within twenty-four hours the relief is complete.

3°. *Iodide of Potassium in Syphilis.*—On the whole, he thinks very highly of it; and having lately, in some obstinate cases, employed large doses—*i.e.*, ℈i. three times a day—he was more than satisfied with the results, and would be inclined to recommend its use to a much greater extent than has been the custom hitherto.

4°. *The "Sulphur Cure."*—He approves of the sulphurous acid spray in cases of chronic bronchitis, with difficulty of expectoration; in cases of advanced phthisis, with much thirst and a parched throat, and in syphilitic and other sore throats.

21. *Morphia—Hypodermic Injection of.*—Dr. Clifford Allbutt especially urges the value of this mode of treatment in the atonic and nervous forms of *dyspepsia*, common in hysterical women; in the dyspepsia of anxious, over-worked mercantile men; and in acute gastric catarrh.—*Practitioner*, June, 1869.

Messrs. Spender and Harrisson report cases confirming the value of morphia, used hypodermically, in obstinate *gastric irritability and vomiting*, particularly in the vomiting of pregnancy, and that resulting from drinking brandy.—*Brit. Med. Journ.*, Oct. 16th; Dec. 4th, 1869.

Dr. Wertheim has adopted this treatment with great success in *bubo*. The bubo is first punctured by a stout needle, and the solution of muriate of morphia injected—at first daily, and afterwards at longer intervals. The fluid that collects should be emptied about every three hours.—*Wien Med. Wochenschr.* Oct., 1868.

Dr. Ravoth has employed morphia injections ($\frac{1}{4}$ gr.) in five cases of incarcerated and inguinal *hernia* as an aid to the taxis, and by

its help readily effected replacement, though the taxis had been previously tried without avail.—*Berlin Klin. Wochenschr.* 23, 1869.

M. Bouloumié has obtained great success in treating *chordee* by this means. He injects $\frac{1}{8}$ gr. at the level of the fourth lumbar vertebra, repeats it the next day, then desists, and, if necessary, resorts to it again. He never had occasion to have recourse to more than four injections. He has also used this plan with benefit in the erections attendant on stricture, on chancrous and other ulcerations of the penis, on operations for phimosis, &c.; and by analogy, recommends its use in congestive dysmenorrhea. In painful *congestive affections of the eyeballs* it is also of great service.—*Gaz. des Hop.*, Jan. 26th, 1869.

Dr. Clifford Allbutt speaks most strongly and decidedly of the invaluable aid to be derived from the morphia syringe in the latter stage of *heart disease*, when other remedies, such as the Prunus Virginiana, &c., are impotent. "No matter how swollen the limbs, no matter how agitated the pulse, no matter how blue and how turgid the face and lips," Dr. Allbutt never hesitates now to "inject morphia, and scarcely ever fails, even up to the time of the dying agonies, to give relief decided enough to earn the warm gratitude of the patient." He begins with $\frac{1}{8}$ to $\frac{1}{6}$ gr. of hydrochlorate of morphia, and in ordinary cases finds $\frac{1}{4}$ gr. the proper dose. The dose is best given in the evening, and should always be followed by perfect quiet in the room. As regards the class of cases in which the morphia is useful, it gives the most striking relief in angina with diseased coronary arteries, &c.; in neuralgic distress from intrathoracic tumour; and in mitral regurgitation. It is very valuable also in small doses, in so-called "irritable heart," but is less useful in disease of the aortic valves. In using the morphia Dr. Allbutt does not wish to overlook other remedies which still have their place.—*Practitioner*, Dec., 1869.

22. *Phosphorus.*—MM. Lebert and Wyss have quite lately shown that phosphorus, when injected, acts far less as a corrosive poison on the gastro-intestinal tract than is generally believed, and that its most formidable toxic action is always secondary and consecutive to absorption. In fact, the lesions of the digestive tube are of little importance, and dangerous accidents only result when the poison is absorbed. Suitable evacuant treatment must accordingly be steadily pursued for some days, *i.e.*, as long as there is risk

of any of the poison being retained in the system.—*Journ. de Pharm. et de Chimie*, Oct., 1869.

In the last Report (*Dubl. Quart. Journ.*, Aug., 1869, Art. 17) a short notice was given of M. Personne's experiments on oil of turpentine as an antidote to poisoning by phosphorus. Since that, in addition to M. Audant's case, Dr. Sorbets records another successful case in which this antidote saved life. Three persons were affected from eating lard poisoned with phosphorus.—*Gaz. des Hop.*, 65, 1869.

M. Personne, anxious to confirm his theory that phosphorus kills by hindering the hæmatosis of the blood, which it deprives of its oxygen, has undertaken some novel experiments with *pyrogallic acid*. This body, though so different from phosphorus, yet, like it, absorbs oxygen with avidity in the presence of an alkaline solution. The acid was administered to two dogs in the dose of two and four grammes, and in fifteen minutes after its ingestion all the phenomena of asphyxia were manifested, just as is the case with phosphorus. Likewise, as in phosphorus poisoning, there was rapid and complete fatty degeneration of the internal organs, and one of the livers was so highly steatosed that it floated on water. M. Personne thinks that these facts leave no doubt as to the manner in which phosphorus kills.—*Rép. de Pharm.*, Oct., 1869.

23. *Phthisis and Physic.*—Dr. R. P. Cotton, after enumerating some of the many so-called specifics which have been vaunted in the treatment of consumption, such as metallic tractors, mud-baths, sleeping in cowhouses, snail-eating, the grape cure, the hypophosphites, and snake excrement, gives his experience as to the value of certain well-known remedies. From pure *phosphorus* he found no better results than from many other tonics and stimulants. *Phosphoric acid* alone and in combination with iron he finds very useful in some cases of cachectic phthisis attended with loss of appetite. The *hypophosphites* he believes to be simply alkaline substances adapted to certain cases of phthisical dyspepsia No decided effect was ever observed from pure *iodine*; but *iodide of potassium* (and especially *iodide of iron*) is a valuable agent in the treatment of phthisis in children. *Arsenic* yielded only negative results. *Iron* is a useful tonic in a great number of consumptive cases; tinct. ferri. perchlor. is the preparation he prefers. *Quinia* is useful as a tonic and antiperiodic, and the *mineral acids* are valuable as tonics. *Alkaline carbonates* are available especially

when there is gastric irritation from acid dyspepsia; *strychnia* gave him but little encouragement; *glycerine* proved of no value, neither did he find any special advantage from *bromide of potassium*, *chlorate of potassium*, or *chloride of sodium*.—*Practitioner*, Aug., 1869.

24. *Quinia*.—Dr. Adolph Martin confirms Binz and Scharrenbroch's conclusions as to the power of quinia directly to arrest the inflammatory process by checking the vital amæboid movements of the white blood-corpuscles, and he has shown that it prevents the actual passage of the corpuscles through the vascular walls in inflammatory conditions, whether given subcutaneously or applied directly to the part. Quinia also hinders the generation of new white corpuscles, and restrains the dilatation of the vessels. Hence its use as an antiphlogistic by reducing all the visible factors of suppurative inflammation, and the explanation of its use in certain pathological conditions, such as typhus, leukæmia, &c. (*Practitioner*). Binz also asserts (*Virch. Arch.*, Bd. xlvi.) that quinia is a powerful antiseptic,[a] more so even than arsenic, both when locally applied or administered internally in septicæmia.

Helmholtz, who was subject to "hay-fever" for 20 years, has found in his own case that vibriones of a peculiar sort occurred in the secretion of his nose, and under the influence of Binz's experiments he injected a solution of quinia (1 to 750) into his nose, and the troublesome symptoms of hay-fever disappeared directly. Next year (1868) as soon as the illness showed itself he again injected the quinia, and the development of the disease was suppressed.—*Practitioner*, Nov., 1869.

25. *Syphilis—Hypodermic use of Mercury*, (See last Report, Art 19.)—M. Aimé Martin has published a careful historical review of this mode of treatment in the *Arch. Gen. de Méd.* for Sept., 1869. Scarenzio, of Pavia, was the first to employ it in 1864, and further experiments were instituted in Italy by Drs. Ambrosoli, Riccordi, and Monteforte, in 1864-65. These surgeons made use of calomel. Hill employed corrosive sublimate in England in 1866, and in 1867-68 Dr. Lewin took up the subject in Germany. Since that time numerous observations have been made, especially in France

[a] It may be objected that solutions of quinia itself sometimes become mouldy, but this is not the case with *neutral* solutions. Sulphuric acid especially predisposes it to this change.

and Germany. M. Martin recommends instead of calomel or corrosive sublimate, which are apt to irritate, a solution of red iodide of mercury in iodide of potassium, while Dr. Bricheteau advises the formula:—Double iodide of mercury and sodium 1 gramme, 50; distilled water 100 grammes. On the whole, we may conclude that the treatment of syphilis by hypodermic injection has produced incontestable good results, and possesses certain advantages over internal mercurial treatment, and that the local accidents which it has sometimes determined are due more to inexperience and a wrong choice of the preparation of mercury injected than to the method itself. Dr. Monti, assistant to Prof. Widerhofer, of Vienna, has treated fourteen cases of infantile syphilis with injections of corrosive sublimate, $\frac{1}{32}$ to $\frac{1}{4}$ grain. With three exceptions the children were cured in from seven to fourteen days; but in seven cases abscesses followed the injections. (*Brit. Med. Journ.*, Dec. 4, 1869, from *Jahrb. f. Kindhk.*) Dr. T. J. Walker records in the *Brit. Med. Journ.* for July 10th, and Dec. 4th, 1869, his experience with corrosive sublimate. In ten cases in which it was fairly tried there was immediate improvement after one or two injections.

CHLORAL.[a]

This remarkable body, which, until very lately, was an object of purely chemical interest for nearly forty years, has, since its announcement as a therapeutic agent by Liebreich, of Berlin, within the past year (June 2nd, 1869), attracted largely the attention of physiologists and practical physicians.

A short summary will here be given of the chief applications of chloral to practice; but in order to understand clearly the theory as to its mode of action, it will be necessary first to sketch its chemical history. In 1832 (1830?), *i.e.*, about the same time as the discovery of chloroform, it was discovered by Liebig, who named it so as to indicate its source, for he prepared it by passing dry *chlor*ine gas through pure anhydrous *al*cohol. This method is the one still in use. Its formula is C_2HCl_3O, or $\begin{matrix} CCl_3 \\ | \\ COH \end{matrix}$ whence

[a] The materials for this article have been gleaned chiefly from the following sources: Lancet, from Sept. 18th, 1869, to Jan. 1st, 1870; Med. Times and Gaz., from Sept. 4th, 1869, to Jan. 1st, 1870; Repert. de Pharm., Oct., 1869; Berlin Klin. Wochenschr, 39-40, 1869. For an opportunity of consulting Liebreich's pamphlet I am indebted to the kindness of Dr. Banks.

it appears that it corresponds to aldehyd (C_2H_4O), with which it has several properties in common, and may be regarded as its trichlorinated derivative. The action of the chlorine on the alcohol is complicated, by-products are generated, and it is a matter of considerable difficulty to obtain the chloral in a state of purity.[a]

Chloral is a heavy, colourless, oleaginous liquid, with a peculiar odour and caustic taste, very soluble in water, alcohol, and ether. It boils at 210°F., and is without action on salts of silver unless a little alkali be first added. It readily combines with a small quantity of water, much heat is extricated, and rhombic crystals of the hydrate $C_2Cl_3OH + H_2O$ are formed. It is this solid *hydrate* which is alone employed in medicine, and to which all the experiments refer. Hydrate of chloral contains 90 per cent. of chloral. When kept for a few days anhydrous chloral is spontaneously converted into an allotropic insoluble form, which has been termed metachloral. From this and other facts it appears that the molecular arrangement of chloral is easily disturbed, and that it is an unstable body.

Chloral is immediately decomposed by alkalies into formiate of the base and chloroform:—$C_2Cl_3OH + NaHO = NaCHO_2 + CHCl_3$. Expressed in numbers this equation signifies that 147·5 grains of chloral + 40 grains caustic soda, yield 119·5 grains chloroform + 68 grains formiate of sodium. This reaction is particularly important, for, according to Liebreich, it gives the key to its physiological action, the alkali of the blood serving to decompose the chloral, so that a *gradual* liberation of chloroform will take place. If this be verified it will be a remarkable and suggestive example of a definite reaction occurring similarly in the living organism as in the test-tube of the chemist.

In reviewing what is known as yet of the physiological action of chloral, it will be more convenient, and will save repetition, to take Dr. Richardson's propositions[b] in order as the standard, and to note the results obtained by other observers according as they agree with or differ from his observations.

Dr. Richardson's experiments confirm, in all essential points, Liebreich's original views. The discrepant statements which have appeared may be, in part at least, explained by the chloral not

[a] A few months since Mr. Spencer Wells paid £3 15s. for two ounces, and Dr. Richardson stated shortly after that it could not be supplied for less than 16s. an ounce. It can now be had in Edinburgh for 6d. a drachm.

[b] Medical Times and Gazette, Nov. 6th; British Medical Journal, Oct. 9th, 1869.

being always of a proper degree of purity, and by the very various dosage adopted, coupled with the anxiety to publish sooner than was compatible with accuracy.

1. "Deep and prolonged narcotism can be safely produced by the hydrate of chloral." All experimenters agree that it is a powerful hypnotic, and according to Demarquay its soporific action is best marked in feeble and debilitated subjects, and the duration of its action is directly proportioned to the degree of debility. In the case of every animal from the lowest to the highest the sleep is induced not merely without pain, but with expression of pleasure. Rabbits arrange themselves to sleep in the most natural manner, and cats purr as they pass into rest in obvious comfort of repose. (Richardson). Its advantages and peculiarities as a hypnotic will be mentioned further on.

2. "During a portion of the period of narcotism there may be complete anæsthesia, with absence of reflex actions—a condition, in short, in which every kind of operation fails to call forth consciousness."

3. "During the narcotism there are intervals of apparent exalted sensibility." Demarquay did not observe the anæsthetic condition, but, on the contrary, exquisite hyperæsthesia along with the hypnotism; but the great weight of evidence is in favour of the production of anæsthesia when a sufficient quantity of the agent is used. The effects, as might be expected, vary with the dose; in small doses it may prove exciting, in full doses anæsthetic, and MM. Dieulafoy and Krishaber assert that we may determine on rabbits *at will*, either hyperæsthesia or complete insensibility by hydrate of chloral, according as a moderate or a large dose respectively is employed. Anæsthetized animals pass through the prior stage of excitement, and sleep exists with hyperæsthesia as well as with anæsthesia. According to M. Labbé, when introduced in sufficient quantity into the blood it produces anæsthesia without preliminary excitement, but when introduced into the alimentary canal or under the skin it produces first sleep, and then anæsthesia. That the mode of administration is not without influence on its effects has been also shown by Richardson in the case of chloroform, for when chloroform is subcutaneously injected in birds, narcotism preceded by no excitement, and succeeded by prolonged sleep and muscular prostration, can be produced. A convincing proof, however, of the anæsthetic action of chloral is afforded by a case published by Dr. Noir in the *Gaz. des Hop.*, Dec. 23rd, 1869, in which amputation

of the leg for osteosarcoma was performed on a man aged sixty-four, while under the influence of 75 grs. of chloral. The insensibility was complete during the operation, and after being placed in bed the patient sank into an alarming coma for one hour, after which, on waking, he was seized with violent delirium and severe vomiting. In about seven hours he recovered his senses, but was greatly prostrated. He spent a sleepless but quiet night, and the next day all the effects of the chloral had passed off.—*Lancet*, Jan. 1st, 1870.

4. "In the transition from drowsiness to stupor there is no stage of muscular excitement, but in birds there is vomiting, as is common in the same animal in the second stage of narcotism from chloroform"

5. "During the narcotism produced by the substance there is invariably reduction of temperature." In birds and rabbits the temperature may fall 5°, 6°, or even 7° F., and yet the animal may recover.

6. "The hydrate produces muscular relaxation, which relaxation extends to the muscles of volition, and also to the iris and muscular arterial system." M. Bouchut concludes that hydrate of chloral is a powerful sedative of the motor and sensitive nervous system, and M. Demarquay stamps it as the most powerful agent we have for inducing muscular relaxation.

7. "The action of the substance on the nervous system is primarily on the sympathetic ganglia, afterwards on the cerebrum, and finally on the heart." Demarquay has shown that in animals (rabbits) put to sleep by chloral the phenomena agree exactly with those observed to follow section of the sympathetic—the ocular and palpebral mucous membrane is injected, and the ears become more vascular though the temperature is lowered. Mr. Barnes noticed in most of his cases slight congestion of the eye and face, and perspiration.

8. "Recovery is followed by no bad results."

9. "In fatal cases the functions are destroyed in the following order: *a*, the cerebral; *b*, the voluntary muscular; *c*, the respiratory; *d*, the heart."

10. "The substance, in small proportions, prevents, in some degree, the coagulability of the blood; and, in large quantities, stops the process of coagulation altogether. In large quantities it also destroys the blood corpuscles."

11. "The phenomena observed correspond with those observed

under chloroform, and the balance of evidence is that they are the result of the action of chloroform." On this point all are not agreed. While Richardson, Personne, Bussy, and others, confirm Liebreich's original view as to the transformation of chloral within the body into chloroform, Labbé and Demarquay do not admit the correctness of Liebreich's position, but they believe that chloral is eliminated through the respiratory organs unchanged, and, according to Demarquay, the peculiar odour of free chloral can be easily recognized in the breath of animals which are under the influence of chloral. But Richardson has shown that chloroform is given off when the hydrate of chloral is mixed with blood, and has succeeded in recovering free chloroform from the blood.

The physiological data which have thus cursorily been reviewed form the best warrant for its therapeutic applications, but in a limited space, and in the present crude state of our knowledge, little more than a mere catalogue of the results of experience can be here attempted.

The hydrate of chloral can be given either by the mouth, by the rectum, or by hypodermic injection. Subcutaneously, some physicians, *e.g.*, M. Laborde, M. Bouchut, and Dr. Ogle have found it give rise to great irritation, whilst others met with no such hindrance. The local irritation was probably due to some impurity in the chloral, *e.g.*, a trace of free acid, and M. Namias by taking care to employ *pure* chloral never observed local mischief, such as eschars, to follow on subcutaneous injection. The solution for injection must be exactly neutral; if not so it should be neutralized by the least possible quantity of ammonia. From 20 centigrammes to 2 grammes have been injected at one time; in the latter case three separate points of injection were necessary. On account of the comparative largeness of the dose so required it seems that administration by the stomach is to be preferred. Chloral may be dispensed in syrup of tolu, or in simple syrup flavoured with tr. aurantii, or in a mucilaginous vehicle flavoured with liquorice, which M. Jastrowitz considers to be the most suitable excipient. In all cases the solution should be well diluted.[a] Generally it is borne well by the stomach, at least at first, but some patients soon evince an unconquerable aversion to it (Jastrowitz), and M. Laborde finds that it is irritant even when taken by the mouth, but when

[a] One of Liebreich's model formulas for a hypnotic mixture is :—℞ Hydratis chlorali 4 parts, aq. distill., syr. cort. aur. āā, 15 parts. M. A table-spoonful to be taken at night.

sufficiently diluted the flavour is not disagreeable. Two grains of hydrate of chloral are equivalent in physiological value to seven grains of chloroform (Richardson), but the dose employed has varied widely in the hands of different observers. For while Dr. Ogle has derived benefit from the use of such small doses as 5 to 10 grs., and found them to act as a hypnotic, as much as 8 grammes (122 grs.) have been given at once without any bad effect (Jastrowitz). Whatever be the minimum dose necessary for an adult it is clear, at any rate, that extremely large doses are tolerated with impunity. If we follow Liebreich the dose of the hydrate of chloral for a child is 7 grs., for an adult the quantity may be extended from 25 or 30 grs. to 100 or 120 grs. Mr. Barnes thinks that 60 grs. is probably the minimum dose requisite to secure sleep, and Dr. Ogle found that 15 grs. of the hydrate produced a good night in a case of scarlet fever after four nights had passed without sleep; 15 to 30 grs. is probably the average dose for an adult to begin with. The weight of the animal seems to influence the action of a particular dose, and in the lower animals there is, we may almost say, a common rule in respect to weight of animal and dose of hydrate; for an animal 3 oz. in weight 1 gr. of the chloral is necessary to bring the animal fairly and fully under its influence. If this be much exceeded the symptoms will end in fatal sleep; this rule holds approximately with the higher animals. (Richardson).

The principal applications of chloral to practice will be noticed, for the sake of convenience, under the heads of relieving muscular spasm, of assuaging pain, and as a nervous sedative and hypnotic. Dr. Richardson suggests that, as chloral reduces the animal temperature, it might be tested with every probability of success in cases where there is a rapid increment of heat, and Mr. W. Adams has tried it with satisfaction in a case of surgical fever following a compound fracture.

1°. *Muscular Spasm.*—M. Bouchut commends chloral as a most prompt and effectual remedy for the severe convulsions of chorea, and Langenbeck has employed it with apparent benefit in a case of traumatic trismus occurring in a boy seven years of age. Dr. Richardson anticipates good results from its use in tetanus especially From some recent experiments of Liebreich, of which Du-Bois Reymond was a witness, it appears that the action of chloral interferes with that of strychnia. A rabbit to which a mortal dose of chloral has been administered, and, after the chloral has begun to

act, a fatal dose of strychnia, very soon revives, never shows a distinct attack of tetanus, and will soon feed as though nothing had happened. On account of the great rapidity of action of strychnia and of the comparative slowness of that of chloral, the converse experiment will not succeed.—*Med. Times and Gaz.*, Dec. 18th, 1869.

2°. *Pain.*—Liebreich expects that chloral will prove indispensable in the treatment of inflammatory painful affections, such as articular rheumatism, and of the various neuralgias, and Bouchut marks it as an anodyne in gouty pains, nephritic colic, dental caries, &c. Dr. Head has tried it in several cases with satisfactory results. He gave 30 grs. in 3 oz. of water, flavoured with tr. aurantii, to a patient suffering from distressing angina pectoris, and decided relief, followed by speedy and calm sleep, was gained. It failed to give ease in a case of tic-douloureux, though it acted as a hypnotic, but in another case of hysterical neuralgia 15 grs. produced easy sleep. Fifteen grs. produced a quiet night in the case of a girl who laboured under mitral regurgitation attended with acute pain in the chest. A patient labouring under aneurism of the aorta, and suffering from intense neuralgic pains, received great relief. In none of the cases were any unpleasant after effects noticed. Mr. Hutchinson has given it in two cases of diseased joint, in doses of 5ss. at intervals of half an hour, with the effect of causing refreshing sleep.—*Brit. Med. Journ.*, Dec. 23rd, 1869.

Sir J. Simpson has seen enough to convince him that chloral will prove a very valuable anodyne in some cases of neuralgia, hysteralgia, and dysmenorrhœa, &c.; and in the pains attendant on cancer and acute local inflammations. He has also found it valuable in some examples of irritable bladder and chronic cystitis. Regarding its influence on the uterus he states, "I have found the parturient uterus to go on contracting regularly and strongly when the patient was so deeply asleep under chloral as to be only very imperfectly wakened up with the expulsive efforts of labour." Sir J. Simpson thinks 50 or 60 grs. a hypnotic dose for adults, but 24 to 30 grs. suffice in some patients. In a case of long standing sleeplessness, 120 grs. failed to produce any effect. As an anodyne, &c., a continuation of smaller doses—10 to 20 grs. several times a day is advisable.—*Med. Times and Gaz.*, Jan. 1st, 1870.

Mr. Spencer Wells found that 30 grs. of chloral procured a night of perfectly tranquil and refreshing sleep in a young lady suffering for the fourth time from a severe and very intractable attack of

sciatica A repetition of the dose was followed by troublesome headache in the morning. In one case of occipital neuralgia in a lady, 30 grs. caused so much burning pain in the throat and stomach that she declined further experiments.—*Med. Times and Gaz.*, Sept. 18th, 1869.

M. Namias has used chloral especially in supra-orbital neuralgia, muscular rheumatism, and hyperæsthesia of the chest in phthisical subjects, and has always found its effects admirable and its action speedy.—*Comptes Rendus*, No. 24, 1869.

3°. *Nervous Sedative and Hypnotic.*—Chloral as a hypnotic is particularly distinguished by *rapidity of action*, ten to thirty minutes being the usual interval between administering the dose and obtaining the desired effect. But in Liebreich's experience sleep has followed in about five minutes after administration, and in one case even as soon as three minutes. In this respect it contrasts favourably with morphia. The sleep is generally tranquil and light, thus differing, as also in duration, from that procured by means of chloroform, and from a large dose sleep is profound and prolonged for several hours.

The sleep it induces is not preceded by the stage of excitement so well known when chloroform is administered by inhalation. This is explained by the gradual extrication of the chloroform within the body. Its hypnotic influence is preserved for weeks without its being necessary to increase the dose (Jastrowitz). In some of M. Demarquay's patients, especially in the case of men, their sleep was agitated and disturbed by dreams, and out of twenty trials six were negative as regards the induction of sleep. Two grammes of chloral in a weakly young woman caused intense headache and a degree of excitation directly opposed to sleep. Unpleasant consequences, even from large doses, seldom ensue, like those that follow opium, and after a perfect sleep from a full dose M. Jastrowitz has observed that patients are usually restored to their primary condition. The lower animals (rabbits) recover completely in a few hours.

M. Jastrowitz contributes a long article to the *Berlin Klin. Wochenschr*, 39–40, 1869, on the therapeutic action of chloral. It was administered to thirty-four patients suffering from various forms of mental and nervous disease, viz.: delirium tremens, melancholia, acute and chronic mania, insanity associated with paralysis, imbecility, &c. The dose varied much, from $\frac{1}{2}$ gramme every hour up to 1, $1\frac{1}{2}$, 2, 4, 6, 7, and even 8 grammes at once.

Only one maniacal patient withstood a dose of 5 grammes. Liebreich thinks that patients labouring under certain mental diseases require in general larger doses than patients whose brain is in a normal state, and reminds us that in all cases regard must be had to the *individualism* of the patient.

Sir J. Simpson has used chloral to a considerable extent in practice, principally as a hypnotic and anodyne. He confirms the favourable opinions of others as to its hypnotic qualities and freedom in a great measure from some of the minor drawbacks and disagreeable accompaniments produced by a full and large dose of opium. It is particularly serviceable in cases where patients cannot tolerate opium in any form, and chloral sometimes produces its hypnotic effects when opium, from its long continued use, has ceased to do so. In a few instances the sleep induced by it has been dreamy and hysterical, particularly when the patient was not kept in a state of perfect quietude (*Loc. cit.*) In delirium tremens especially the testimony to its value is very decided by Jastrowitz and Langenbeck (*Berl. Klin. Woch*, 35, 1869), and Mr. Barnes reports favourably of its advantages as tested in ten cases of delirium tremens in the Liverpool Workhouse Hospital.—*Lancet*, Nov. 27th, 1869.

In a case of violent delirium following a comminuted fracture of the humerus in a woman aged forty, of intemperate habits, cessation of the delirium and tranquil sleep were brought on by from 2 to 6 grammes of chloral, after 7 grs. of opium and $\frac{3}{4}$ gr. morphia had proved unequal to quieting the patient.

Mr. H. T. Chapman employed chloral most beneficially in a severe case of delirium tremens occurring for the sixth time. Thirty grains of chloral induced sleep in less than five minutes, and on waking, after nearly an hour's sleep, he was quite composed and rational; 20 grs. more caused several hours refreshing sleep, and he soon convalesced.—*Med. Times and Gaz.*, Oct. 2nd, 1869.

Dr. Llewellyn Williams and Mr. Kesteven have also found much advantage from the administration of chloral in delirium tremens in small and repeated doses.—*Journ. of Ment. Science*, Jan., 1870.

Since in anæsthetic virtues chloral does not compare favourably with the other volatile anæsthetics, and is not likely to supersede chloroform, at least in the performance of capital operations, the sum of the experience which has been hitherto recorded seems to point to the hypnotic and anodyne powers of chloral as its most valuable qualities in a therapeutical point of view. If it fulfils half the expectations which have been formed of it it will be a strong

point in favour of those who advocate the importance of organic chemistry to medical science, and look to it for light and assistance in the progress of therapeutics. It is a striking example of an agent introduced into practice on the basis of a theoretical estimate founded on a precise knowledge of its physical and chemical properties. Looking back on all the facts before us we notice on the whole an unusual and surprising concordance of results among the many observers, and allowing for certain disturbing circumstances, inevitable at the outset, we have every reason to believe that Liebreich has added a really valuable agent to our therapeutic list.

PART IV.

MEDICAL MISCELLANY.

Reports, Translations, and Scientific Intelligence.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

THIRTY-FIRST ANNUAL SESSION.

DR. RINGLAND, President.

GENTLEMEN,—This night, which is about to close my year of office as your President, will terminate the first year of the fourth decade of this Society's existence. Thirty-one years have run their span since the foresight, genius, and energy of its founder brought it into existence as a means of filling a want previously existing and long felt, and of fulfilling an object of essential moment to the scientific progress of our art in this country—namely, a common arena where we could communicate to each other our views and the result of our individual experience, where our theories might be investigated, our crude notions eliminated, our facts tested, our practice developed and applied; in fine, where we could take friendly counsel together, and where, with that mutual forbearance and perfect unanimity which ever excludes a spirit of contentious sarcasm, we could become an assistance each to the other in the applying of what is known to aid in the discovery of what is unknown.

In addressing you at the close of my year of office, I am but fulfilling one of the duties of the high position which I hold through your kindness and consideration; yet it is with no small amount of diffidence I have applied myself to the task, not merely feeling an apprehension that I should fail in its suitable performance, but also finding extreme difficulty sin electing a subject which would be appropriate to the occasion and to the mixed auditory who honour us by their presence at that special meeting of the session to which alone the general public are admissible.

Moreover, I feel myself in a position of no small difficulty, the ability of my predecessors in this chair having left me no easy task to follow—*cum longo intervallo*—in their footsteps. If, however, I cannot in every respect do all that they have done, I shall yield to none in an anxiety for

the future welfare of the Society, and for its continuous and persistent progress in the development of its objects.

To the annals of midwifery in Ireland, and of its collateral branches "the diseases of women and children," I purpose, in some measure, to direct your attention; to do so fully would occupy volumes and weary my hearers. In compiling these data, I have largely availed myself of the labours of Sir William Wilde and of Drs. Evory Kennedy, Thomas E. Beatty, Aquilla Smith, Alfred M'Clintock, and Thomas William Belcher, who have published the result of their learned researches, and have left but little to be gleaned by those who might follow them in the same field. To these I must add another name, that of Professor Travers, so well known and highly appreciated as a black-letter scholar by all who are acquainted with him, and who is so conversant with ancient Irish literature, he some time since, and for another purpose, supplied me with valuable notes, of which I have fully availed myself on the present occasion.

No doubt, it may appear to many that the task I have undertaken is altogether unnecessary—is, in fact, a work of supererogation;—that each individual I have now the honour to address could easily obtain, in a more agreeable form, all the information now submitted, by a reference to the original papers themselves; yet still I conceive that there are but few who will take the trouble to search for themselves; and moreover that it may be useful occasionally to accumulate facts, and recall past events for the information of the many who will not weary themselves by personal research, in the hope that the knowledge of the many obstacles that have been overcome, and of the victories gained by the industry and ability of the great men who have preceded us, despite the difficulties and drawbacks they encountered, may stimulate us to emulate them, and thereby preclude our noble profession from deteriorating in the hands of any one of us.

That no man's history can be written during his lifetime is an aphorism so universally received that it requires from me mere assertion as a truism. It is with truth said that we are charitable to the memory of the dead, but the same benevolent feeling is not always extended to the living. To touch, then, upon living celebrities, no matter what good service they have done, or to what extent they deserve well of their generation, or upon the events enacted by them, is a task so invidious that it would be evaded by most. I wish it, therefore, to be understood from the outset that it is my present purpose to confine my sketches exclusively to the history of the past, and not to touch upon men of the present day, excepting in one or two instances, wherein some coincident circumstance may unwillingly lead me to a trivial deviation from this determination.

It must be borne in mind that antecedent to the Christian era, if we except the classic writings of Greece or Rome, there existed but little or

no written literature, and that consequently the history of a country was preserved through the medium of the memory, facts, or traditions, as the case might be, having been transmitted from individual to individual, and from generation to generation.

Even when literature had made some progress amongst the Greeks and Romans, any allusion to Ireland is of the vaguest character. The fabulous Argonauts are described as having visited the two islands of Great Britain and Ireland, which are named Irenides (νήσοισιν Ἱερνισεν); and Aristotle, in his treatise *de mundo*, refers to Albion and Ierne (Αλβιον και Ιερνε); and the Greek geographer, Strabo, only refers to Ireland to mention the coldness of its climate and to comment on the barbarism of its inhabitants.

Many other authors subsequent to these likewise make allusion to Ireland; but I merely refer to the matter in general terms to show that if that island was not appreciated in the early history of the world, it was at least not altogether unknown; but under such circumstances as I have already referred to, it cannot be expected that the early records of Irish medicine are much to be depended on.

Physic among the ancient Irish was an hereditary possession, transmitted from father to son, as in Egypt; and this system continued to a comparatively recent period; thus, we find no infrequent mention of the O'Reilly's and O'Sheil's as members of the faculty.

The ancient Irish possessed much medical knowledge, combining a practical acquaintance with the native simples, and the doctrines of the Greek and Roman physicians. Several operations, such as cupping, venesection, and the employment of the seton, have been traditionally preserved amongst the peasantry, having been handed down from the earliest period, and are to this day practised throughout many country districts.

It is difficult to fix a date for the first foundation of hospitals; it is believed that they were originally brought into existence by the Buddhists in Hindoostan, but this fact was utterly unknown throughout Christendom in the early centuries of the Christian era, and consequently could not affect the establishment of such therein. In their earliest career in the western hemisphere they were connected with monasteries and religious houses: and the inmates of these not merely exercised the gift of hospitality but frequently afforded medical and surgical aid to all claimants for such assistance. The following extract from the interesting Life of St. Columba, by the Rev. Dr. Reeves, published in the *Transactions of the Archæological and Celtic Society*, 1857, pages 55–56, demonstrates the existence of such an arrangement in the monastery of St. Columba, and it is not without interest to know that so early as the close of the sixth century hospital accommodation was afforded, through the intervention and foresight of an Irishman, to the sick and needy. The quotation is

from the chapter:—"de aliquo miserabili viro qui ultra supradictum calamitabat fretum," or, as expressed more explicitly in the "Capitulationes" or Summaries, p. 11, "de aliquo misero viro, qui ad fretum calamitabat, mox morituro."

"Quadem quoque die, quemdam ultra fretum audiens calamitantem, sanctus, hoc profatur modo: valde miserandus est ille clamitans homo, qui, aliqua ad carnalia medicamenta petiturus pertinentia, ad nos venit: cui opportunius erat veram de peccatis hodie pœnitudinem gerere; nam in hujus fine hebdomadis morietur." There is the following foot note, by the Editor, attached: "It would seem from this that St. Columba's Monastery was resorted to for the relief of bodily infirmities."

We do not find it recorded that any medical faculty existed in the Mur-Ollam-ham, the University or College of the learned, founded at Tarah, about 760 years before the birth of our Saviour, by Eochair, the then King of Ireland; yet it is not unreasonable to suppose that such may have been established by that learned monarch, who from his great abilities had been designated Ollamh-Fodlah, or Doctor of Ireland; this great legislator undoubtedly established the usage which made employments and offices hereditary in families, to each of which he assigned lands for the maintenance of the holders; and there exists little doubt but that amongst the most important offices thus transmitted were those of physicians; of this, however, it is certain that about 600 years after the foundation of the Tara University, of such high repute were the physicians and surgeons of Ireland, that Iosina, afterwards King of Scotland, was, with his parents' permission and approbation, entrusted to them to be educated.

We know nothing of the state of medicine among the native Irish for several centuries subsequent to the English invasion, except what can be gleaned from the very few manuscripts which still exist; and we cannot too strongly deprecate the oversight of our colleges and scientific bodies, in neglecting an opportunity which some years since presented itself, of procuring possession of a considerable collection of unpublished Irish writings, which might have thrown light on this interesting subject, or afforded some clue to its elucidation. There are some most valuable Irish medical manuscripts to be found in the libraries of Trinity College, the Royal Irish Academy, and the British Museum; also in the private collection of the Earl of Ashburnham. These last were carried out of Ireland by the Rev. Charles O'Connor, D.D., librarian to the Duke of Buckingham, in whose library he placed them, and from which they, some time since, passed into the possession of their present owner. The removal of these Irish manuscripts from this country drew from the pen of the celebrated Dr. John Brennan one of his most able but caustic letters, which was addressed to Dr. M'Dermott, the Prince of Coolavin. Dr. O'Connor compiled and edited several books on Ireland; among others,

Rerum Hibernicarum Scriptores, 4 vols., 4to, 1814-26; and a catalogue of the manuscripts in the Stowe library, 2 vols., 4to, 1818-19.

Most of the Irish manuscripts were written in the Irish language, but many of them in Latin, in the Irish character. From a study of these, and of these alone, many practitioners acquired the only knowledge of medicine they possessed, and upon which they subsequently practised. Whitelaw and Walsh, in their *History of Dublin*, state that comparatively recently, one O'Casey, residing in Mitre-alley, practised medicine with no other professional knowledge than that obtained from this source.

It is much to be regretted that but few of these manuscripts have been given to the world in an available form; but it is to be hoped that the efforts now being made by some of the friends of the late Rev. Dr. Todd, who devoted so much of his life to the study of Celtic literature and antiquities, may be crowned with success, and that the professorship they contemplate in connexion with the Royal Irish Academy may be speedily founded. We then shall hope that the rich treasures which undoubtedly exist in these manuscripts may be brought forth from their respective receptacles, and submitted, in an intelligible form, to the investigation of every student who desires a knowledge of their arcana.

On the revival of literature in Europe, Irishmen, both at home and abroad, added their quota to the publications of the day. In 1538 Dr. Thadeus Dean issued from the press a volume, *De Morbis Mulieribus*, in which he mentions a case attesting the efficacy of the medicated warm bath in tedious labour. It is interesting and instructive to look back these more than three centuries, and observe that what in modern days is looked upon as one of the best and most approved plans of treatment, should have been so long anticipated by one of our countrymen. Well may we say—"Verily, there is nothing new under the sun."

The celebrated Robert Boyle, facetiously styled by a lecturer at the Royal Dublin Society, "the father of chemistry and the son of the Earl of Cork," was born at Lismore in 1626, and from a very early period of his life employed his gifted and philosophic mind in innumerable scientific researches; amongst these, transfusion engaged his attention, and to his other manifold studies he added that of anatomy; and his observations as to the formation of the chick and fetus are by no means unworthy of attention. This knowledge of the structures and physiology of the human frame did not mislead him from the appreciation of the divine powers of its Maker, but led him ever from the study of nature to the adoration of the God of nature. "I satisfied myself," he writes, "of the circulation of the blood, and have seen more of the variety and contrivances of nature, and the majesty and wisdom of her Author, than all the books I ever read in my life could give me a notion of."

On the 8th of August, 1667, was incorporated the Irish College of Physicians. The celebrated John Sterne, or Stearne, Doctor of Laws

and Medicine, was its founder and first President, and his name "Johannes Stearne," stands first on the roll of its Fellows. He died on the 6th November, 1669, and his remains were interred in Trinity College, outside the chapel of which, on the north-west side, stands his monument. He made no contribution to obstetric literature, and is referred to here as the founder of an institution, which, however great has been the impetus it has given to general medical science since its foundation, has certainly, during the early years of its existence, and even up to a comparatively recent date, given no encouragement to the advance of the science of midwifery or to its professors.

This charter of Charles II. contained no provision whatever on the subject of midwifery. It was surrendered, at the request of the College, on the 14th of December, 1692, and on the following day a new charter was granted by King William III. and Mary his queen; and hence the name, "the King and Queen's College of Physicians in Ireland," by which the College has been since and is still known. By this charter the College was entrusted with the entire and absolute supervision of midwives, and with authority to examine and licence in midwifery. In the exercise of this power the first licence in midwifery was granted on the 3rd February, 169$\frac{6}{7}$—four years after its amended incorporation—to Mistress Cormack, who seems to be the only woman licensed by the College, who reserved their favours to the other sex for eighteen years more, having granted its second licence in midwifery on the 21st of October, 1715, to Dr. James Hamilton, surgeon.

It is not, to my mind, by any means improbable that the power already stated as vested in the College by the charter of William and Mary, to supervise midwives and to examine and licence in midwifery, combined with the low estimate in which obstetric practitioners were held by the physicians and surgeons of the day, may have originated the phrase "midwives," as applied to those medical men who practised the obstetric art. Of late, however, except as an occasional jest, this offensive appellation has been abandoned and good taste has excluded its use among professional gentlemen.

Despite, however, the assertion by the College, as already stated, of their right to licence and control practitioners in midwifery, a narrow-minded policy appeared to pervade all the acts of the College about this period. On the 19th of February, 1753, they passed a bye-law to the effect that no practitioner in midwifery should be examined for a medical degree or for the licence in medicine of the College; and acting on this bye-law the College, in 1760, refused to comply with the request made by the University of Dublin—to examine Mr. (afterwards Sir) Fielding Ould for the degree of Bachelor of Physic of that University, although he was one of their own licentiates in midwifery since the 16th of August, 1738, and despite the fact that his admirable treatise had merited

and received a testimonial of approval from the College under the signature of its President and Censors, on the ground that the candidate was by his calling disqualified for obtaining a medical degree. The University, however, actuated by that liberal spirit which has usually influenced its actions, had the candidate examined by its own professors of physic and medical lecturers, and gave him the degree of Bachelor of Medicine on the 27th of Jan., 1761.

It might be imagined that this liberal course on the part of the University would have produced a corresponding beneficial effect on the College of Physicians. On the contrary, however, illiberality continued to prevail for many years; and it was with no little difficulty that practitioners in midwifery could procure a recognition of any kind from the College, *a licence in midwifery* being all that they would grant, and that only to a chosen few: and it was not until many years after the period I have already referred to that *the licence* of the College was accorded to them, Dr. Francis Hopkins, author of *The Midwifery Vade Mecum*, and at the time Master of the Dublin Lying-in Hospital, being the first upon whom that honour was conferred. His admission bears date the 1st November, 1779.

If the College was tardy in granting their merited position to obstetric practitioners, they thenceforward endeavoured to remove the unjust stigma they had previously cast upon them; for we find from this time forth they were admitted not merely to its licence but also to its Fellowship. Dr. Hopkins, on the 17th of April, 1780—less than six months after he was admitted a licentiate—having been elected to the higher honour; and on casting our eyes down the list of its members we find many honoured names, including that of Sir Fielding Ould, who, despite the previous opposition offered to him by the College on account of his calling, was enrolled one of its licentiates on the 3rd of October, 1785.

That the College was not altogether unmindful of the importance of midwifery as a branch of medical studies, even whilst their conduct toward its practitioners was so illiberal, is demonstrated by the fact that in the Act of Parliament passed in 1743 (the 21st of George II.), which makes provision for the expenditure of the proceeds of Sir Patrick Dun's estates, a Professorship of Midwifery in conjunction with surgery was founded; and on the 20th of May, 1749, the first and only Professor of Chirurgery and Midwifery under this Act was elected—namely, Dr. (afterwards Sir) Nathaniel Barry. This eminent physician was admitted a Fellow of the College on St. Luke's Day, 1758, and was elected its President in 1767, and again in 1775. He died in 1785.

Subsequent to the appointment of Sir Nathaniel Barry a retrograde policy appears to have prevailed, for we find that the 25th George III. abolished the professorship of chirurgery and midwifery, and the

40th George III., commonly known as the School of Physic Act, passed in 1800, made only conditional provision for a professorship in midwifery. The College, however, becoming more alive to the importance of this subject, did not await the accomplishment of the specified condition, but in 1827 determined to appoint a professor on the College foundation, and elected as such one of the most accomplished physicians that ever adorned the Dublin School—the late Dr. William Fetherston Montgomery. This office he held until his resignation in 1836, when he was succeeded by Dr. Fleetwood Churchill, and on his resignation in 1864, Dr. Edward Burrowes Sinclair was elected thereto. By the recent Act, 30 Vic., cap. 9, 1867, this professorship has been placed in the position to which it was justly entitled, its holder having been thenceforward created a King's Professor.

It will be seen from the statements I have here made, that for many years not merely have all the offensive restrictions to which I have already referred been removed, but that the highest rank the College can offer to any of its members is gladly accorded to the midwifery practitioner; moreover, a knowledge of the science itself has been deemed by them so essential to every medical man that one of the Censors of the College is appointed to examine in this specialty *every* candidate for its "licence;" whilst those who desire the recognition of the College as midwifery practitioners must submit themselves for examination before the Special Midwifery Court, without whose approval the "midwifery licence" cannot be obtained.

But I have wandered much from a chronological arrangement of my data. I must therefore call back your attention many years.

In the *Transactons* of the Dublin Philosophical Society, which was founded in the year 1683, and was the great prototype of all our existing learned bodies, but particularly of the Royal Irish Academy, the following papers were published within the first four years of its existence. On "Ovarian Disease," by Dr. Allen Mullen, or Moullen, who was the first medical man of eminence who published in these *Transactions*; on "Hermaphrodism," by Mr. St. George Ashe; and by the same author, "An Account of a Man in Galway who succled his child, and had Pendulous Mammæ;" a second essay on "Hermaphrodism," from the pen of Dr. Willoughby appeared in the same *Transactions;* and one "On the Dissection of a Monstrous Child with two Heads and three Arms," by Dr. Houlaghan.

In 1698 Dr. (afterwards Sir) Thomas Molyneux proposed the extraction of the stone from the female bladder without excision. This learned physician was Professor of Practice of Physic in the University of Dublin, and was one of the Fellows of the College of Physicians named in the charter of Charles II. He was elected President of this College in 1702; again in 1709; for a third term in

1713; and finally in 1720. Sir Thomas Molyneux was the first medical baronet ever created in Ireland; nor was there another for one hundred and nine years afterwards, when Sir Henry Marsh—a lineal descendant from the subject of this memoir—and Sir Philip Crampton had each the honour conferred upon him; and there was again an interval of several years before a similar well-deserved compliment was paid to Sir Dominic John Corrigan. In 1728 Sir Thomas Molyneux retired from practice, with a splendid fortune, realized through his talent and professional industry. Archbishop King passed on him this high encomium—to which all who were brought within the sphere of his acquaintance admitted that he was justly entitled—that he was "the most eminent physician in this kingdom, yet not more remarkable for his skill in his art than for his piety and virtue." "The high eminence," says Sir William Wilde, "to which he attained, and the esteem which he earned for the medical profession in this city, as well as the love of country which characterized all his pursuits, and gave a tone of patriotic enthusiasm to all his writings, justly entitle him to the appellation of "the father of Irish medicine."

In 1698 Dr. Bernard Connor, Physician to John Sobieski of Poland, wrote a curious discursive treatise entitled *Evangelium Medici*, in which, whilst treating of the suspensible laws of nature, he anticipated John Hunter's views as to the possibility of human generation being effected *sine concubitu*. It may not be out of place here to mention, and it will, probably, be in the recollection of some of those whom I now address, that in the year 1841 Mr. Donovan submitted a most interesting communication, propounding the same theory, to the King and Queen's College of Physicians, at one of its evening meetings. Dr. Connor, like too many of the most promising of our Irish authors, died young, and "far away from his dear native land."

The Philosophical Society having for some time ceased to exist, was revived in 1707. In the *Transactions* of the re-organized Society there is detailed a most interesting case, wherein Dr. Thomas Proby, the principal surgeon of Dublin of that day, extracted a bodkin from the female bladder. This gentleman was "Chirurgeon-General," and was the ancestor of the Carysfort family. A case of "spina befida" is likewise detailed in these *Transactions* by Dr. John Rutty, a licentiate of the College of Physicians.

Up to this date, it must be borne in mind, that the practice of midwifery was mixed up so completely with the practice of medicine and surgery, that it is quite impossible at the present time to trace out any distinction, and that the treatises and practices I have already referred to were those of practitioners of medicine who devoted themselves *in some measure* to the collateral branch—midwifery. Public prejudice had hitherto precluded its practice by male accoucheurs; and it was only in very

occasional cases of extreme difficulty that their assistance was at all permitted. In the early part of the eighteenth century, however, the dark mist of ignorance which had for so long enveloped the human race began to clear, knowledge and literature progressed, and the whole age became more enlightened. As the mind of the female sex became more expanded, the management of all cases in the least degree deviating from what was natural, was confided to the male practitioners; and thus the facilities for acquiring practical knowledge increased, and therewith, also, their skill and experience, as proved by the greatly diminished mortality of both mothers and infants. From this period, therefore, may be dated the practice of midwifery as a distinct branch.

In 1742 Sir Fielding Ould, to whom I have already made reference, and who, many years afterwards, was Master of the Dublin Lying-in Hospital—having been elected to that office in November, 1759—published his *Treatise on Midwifery*, which was the first work of the kind ever published in Ireland, and laid the foundation of the now well established literary reputation of the Dublin School of Midwifery; moreover, "if we except," says Dr. M'Clintock, "the writings of Chapman and Sir Richard Manningham, it was the first obstetric treatise having any pretensions to merit and originality which appeared in the English language." In this work he has well described the progress of the head in natural labour, and explains the development of the uterus under the influence of pregnancy. He likewise advocates the operation of version in cases of slight deformity of the pelvis, and on this point certainly merits the credit of originality. He invented the "terebra occulta," which has, however, fallen into disuse.

Sir Fielding Ould complained, and with great justice, that although there were at this time in Dublin as good physicians and surgeons as were to be met with in the world, and who had been educated in their own country, yet that they were compelled to seek in France a true knowledge of practical midwifery, as there alone they had the opportunity presented to them of witnessing ocularly the delivery of women in both natural and preternatural labours.

Shortly after the last date referred to, a new era arose in midwifery practice, in the foundation of the Dublin Lying-in Hospital. Dr. Bartholomew Mosse, one of the most humane, energetic and enlightened of men, determined to meet the evil Sir Fielding Ould had merely deplored, and with that object vigorously set about the establishment of an hospital which would have the two-fold advantage of giving relief in nature's most trying hour to the destitute females of this metropolis, and of furnishing practitioners in midwifery with the means of instruction.

It may not be without interest here to mention that Dr. Mosse was born in Wexford, in the year 1712; the family, however, of which he was a member, resided principally in the county Carlow. He has been occa-

sionally styled surgeon and man-midwife; his diploma as the former was granted, according to the then existing custom, by the Surgeon-General; whilst his licence in midwifery was obtained from the King and Queen's College of Physicians, on May 3rd, 1742.

Dr. Mosse made his first essay in his previously well-digested plans about the year 1745, by renting a house in a back lane off George's-lane—now known as South Great George's-street—and opening it as a lying-in hospital. This house still stands, although in a dilapidated condition, and is pointed out to the curious stranger. The encouragement he met with induced Dr. Mosse to proceed, and the noble institution which now stands in Great Britain-street—one of our City's greatest ornaments in every sense of the word—was erected under his auspices, and on December the 8th, 1757, was opened for the admission of patients under the sanction of a charter which had been obtained the previous year from George II. From that period down to the union of this country with England, annual grants of large amount were voted by the Irish House of Commons for its support. Since that time, however, the parliamentary grants made by the British House of Commons dwindled down from less to less, until eventually all such grants were totally withdrawn, as well from this as from each of the other Dublin hospitals. On the restoration of the grants some years afterwards, the small sum of £600 a-year was accorded to this noble charity. It is gratifying, however, to know that its income from private sources is considerable, although in some measure inadequate to fulfil all its governors would desire. Dr. Mosse was not destined long to enjoy his well-earned reputation, as he survived the accomplishment of his great work only two years. He died at the comparatively early age of forty-seven, on February the 16th, 1759, worn out by mental and bodily fatigue. Of him Sir William Wilde writes:—"Thus died this great and good man, in the prime of life and usefulness, possessing great energy and high benevolence of character, which he so employed as to have conferred innumerable benefits upon his own and subsequent generations. His eulogy is to be found in his acts. Without fortune, without influence, without patronage, without precedent, he conceived the project of affording relief to a certain class of the community; and, with extraordinary energy, prudence, and perseverance—never relaxing, never despairing—he carried it into execution at an expense of character, station, and pecuniary independence." "May we not," says Dr. M'Clintock, "truly apply to him the beautiful language of Grattan, and say, 'that in feeding the lamp of charity, he had exhausted the lamp of life?'" He was the first Master of the Hospital, which at its opening contained but one hundred beds; but now, with the aid of an additional wing, maintains one hundred and thirty; and it may not be out of place here to mention that several of these extra beds are appropriated

to the treatment of cases of "female diseases," a ward for that purpose having been originated through the untiring energy of Dr. Evory Kennedy during his mastership of the hospital; and thus a great boon was granted to the suffering female, and a teaching power never anywhere before in existence added to the student's *repertoire*. The hospital remains the proudest and most enduring monument to the memory of the "great and good man" who founded it that human art could devise; and although no monumental brass points out his resting place, we have but to glance at the handsome structure erected through his untiring exertions, and recall the noble cause to which he devoted it, and repeat within our inmost breasts, "*si queris monumentum, circumspice.*"

When Mosse was about to pass away, another able obstetrician, whose name well merits enrolment amongst those who have adorned the character of the midwifery practitioners of our City, was but then buckling on his armour for the "battle of life." Dr. John Charles Fleury was a most accomplished classical scholar; he commenced practice in both medicine and midwifery in 1760, immediately after he had obtained his medical degree in Edinburgh, and being then in his twenty-seventh year. Three years subsequently he was elected physician to the Meath Hospital, and held that appointment up to 1786. In 1784, he was elected a licentiate of the King and Queen's College of Physicians, "without examination or expense"—a most exceptional compliment, but one well merited by its recipient. Dr. Fleury submitted many valuable papers to "The Medico-Philosophical Society" of Dublin, of which he was a member. In one of these, communicated on December 7th, 1775, he pointed out what we now receive as the most reliable sign of ruptured uterus—and that ten years antecedent to the publication of Dr. Andrew Douglas's celebrated *brochure* on that subject—"complete retrocession of the head after it has been for some time presenting." In December, 1777, he read before the same Society, an interesting paper, detailing a case of retroversion of the uterus occurring in the third month of pregnancy.

It will be borne in mind that I have already mentioned the fact that Sir Nathaniel Barry had been appointed Professor of Surgery and Midwifery conjointly in 1743 by the King and Queen's College of Physicians; very little doubt, however, exists but that the former subject occupied the lion's share of the attention of the Professor, and that the latter was permitted to remain very much "out in the cold." About 1761 or 1762 Dr. Fleury commenced systematic courses of lectures on "Midwifery and the Diseases of Women and Children," which he continued up to 1769. It is worthy of observation that these lectures were delivered in the Anatomical Theatre of Trinity College; and although the Board of the University did not grant direct permission for the delivery thereof, yet the indirect sanction they accorded demonstrates the liberal feelings which influenced that distinguished body. The way this came

about is thus described by Dr. M'Clintock, "Fleury applied to the Board, through Dr. Mercier, one of the Senior Fellows (and afterwards Rector of Omagh), for permission to use the Anatomical Theatre as a lecture room; which request the Board refused to grant 'unless Dr. Fleury produced a testimonium or certificate, signed by the Medical lecturers upon the College foundation,' (who were then Drs. Hutchinson, Cleghorn, and Span) 'that his proposed lectures were likely to be of use, and that they thought him qualified to give them;' which certificate they readily furnished." It thus appears that to Dr. Fleury belongs the merit of having been the first systematic lecturer in Ireland upon "Midwifery and the Diseases of Women and Children."

Another point of no little interest in connection with Dr. Fleury's obstetric career is, that with the view to render his lectures truly practical, "he was in the habit of attending, with his class, poor women in labour at their own homes," and thus initiated a system subsequently found to be of inestimable value by aiding the poorer classes who are unable to avail themselves of intern hospital accommodation.

Dr. Fleury died suddenly on September 29th, 1797, aged sixty-four, universally regretted; he had some years previously retired from practice.

I must not omit here to mention the name of Dr. David MacBride, who practised midwifery about this period; although, however, he is the author of standard works in medicine, he has not contributed anything to the literature of midwifery. This is the more to be wondered at, as, on the decision of the Governors of the Lying-in Hospital to establish stated courses of lectures having been arrived at, he was elected the first lecturer, but did not enter on his duties until the latter end of 1774, or early in the following year, and he is supposed to have continued in the persistent performance thereof until the period of his last illness in 1779; and I refer to him here chiefly because he is alleged to have assisted at the birth of "The Hero of Waterloo;" my friend, Mr. Travers, has seen a manuscript note to that effect in a quarto copy of his *Practice of Physic*, in the hand-writing, as he conjectures, of Dr. George Cleghorn. Yet, still, that distinguished honour has been claimed for Sir Fielding Ould, and, I think, correctly; Dr. MacBride's own biographer, in the *Dublin Quarterly*, clearly proving that there exist no grounds whatever for such a supposition, whilst Dr. M'Clintock writes—"From certain facts with which I have been made acquainted by some of Ould's immediate descendants, I entertain no doubt whatsoever that he it was who attended the Countess of Mornington at the births of the Marquis of Wellesely, and of the still more illustrious Arthur, Duke of Wellington." Dr. MacBride died in 1779 or 80.

On the termination of Sir Fielding Ould's mastership of the hospital, in November, 1766, one of his assistants, Mr. Henry Collum, was elected

to the vacant office. It was about the year 1770, while he was master, that the advisability of having systematic courses of lectures on midwifery delivered within the institution first occupied the attention of its Governors. This subject was warmly discussed not merely within but also without the hospital, many able writers having been arrayed on either side of the question; and it was not for some time afterwards that the system was definitively adopted.

In 1770, Dr. Jebb published an interesting work entitled, *A Physiological Inquiry into the Process of Labour.* This, I believe, was the only emanation from the pen of this accomplished physician, which is the less to be regretted as the theories he therein propounded reflected but little credit on his obstetric acumen. Dr. Jebb was elected Master of the Dublin Lying-in Hospital in November, 1773, and was the first who resided in the hospital.

In 1781 was posthumously published a *Systematic Work on Midwifery*, written by Dr. Foster, who had been an assistant in the Dublin Lying-in Hospital. In this work—which was edited by Dr. James Sims, of London—the author treats of puerperal fever, and questions the correctness of its nosological epithet, conceiving it possible that a similar disease may occur in the male, and affirms his having met with two such cases. How singular, that after the lapse of considerably more than three quarters of a century, our present experience enables us to confirm this, then strange doctrine.

Mr. William Dease, one of the most experienced surgeons of this city in his day, published, in 1783, *Practical Observations on Midwifery*, including different methods of assisting women in tedious and difficult labours. He also published an essay on *The Disorders of Women and Children.*

The Royal College of Surgeons in Ireland received its first charter of incorporation in 1784. This body—taught experience, in all probability, by the narrow-minded policy of the College of Physicians, and influenced by more liberal principles—obtained authority, by this Charter, to grant licences in midwifery to its own licentiates—but to them only—and from almost the very outset acted upon this authority,[a] and thus an

[a] "Among the first professorships created in this College," writes Dr. M'Clintock, "was one of Midwifery, according to resolutions adopted in August, 1785. Through want of financial means these resolutions were not carried into effect till 1790, when the School of Surgery opened in good earnest, and Mr. John Halahan, the Professor of Anatomy and Physiology, and an ex-assistant of the Dublin Lying-in Hospital, began regular courses of lectures upon Midwifery in the College School, and continued to do so till 1793, when the College deemed it expedient to appoint a special professor to the chair of Midwifery, and for this purpose they selected another ex-assistant of the Lying-in Hospital, Sir Henry Jebb, who was an active member of the College, and subsequently its president. From that time to the present there has been an unbroken succession of obstetric professors, and of regular courses of lectures on Midwifery in he College School."

additional impetus was given to the progress of professional knowledge in the theory and practice of obstetrics; and it may be confidently asserted that the sound teaching afforded by the professors of that College, has produced abundant fruit, from this body having emanated some of our soundest practitioners; and whilst, to its exertions, we are indebted in a great degree for the proud position the Surgical School of Dublin now holds as second to none in the world, midwifery has derived many champions from its ranks, who have reflected honour on the body which had eliminated their education, and on the country of their birth.

I have now brought you to the close of the eighteenth century, during the last sixty or seventy years of which, midwifery distinct from either medicine or surgery, was practised by but a few, scattered over a prolonged period, and appearing almost, as it were, in succession, but frequently, *cum longo intervallo*. Within the last sixty or seventy years, however, it has made great strides as a science; many of our most discerning, soundest and ablest medical men have devoted themselves exclusively to its practice, and by their ability and research, coupled with zeal and honourable conduct, have rendered the Irish midwifery practitioner universally popular, and have raised that branch of the profession to the high status it now so deservedly holds.

Foremost amongst this celebrated band stands Dr. Joseph Clarke, who first applied the science of statistics to midwifery practice. In the report of his seven years' Mastership of the Dublin Lying-in Hospital—to which he was elected in the year 1786, on the death of Dr. Rock, who died in office—he gave to the world a brief but succinct statement, which has formed a copy, frequently followed since. His able researches and successful treatment of *trismus nascentium* have stamped him as an able author and a sound practitioner.

At the time to which I now refer, this disease—the inmate of badly ventilated institutions wherein infants are crowded together, and but rarely met with elsewhere—was the opprobrium of the Dublin Lying-in Hospital, sweeping away annually an incredible number of victims. The nurses of that institution would not carry out any system of ventilation; carefully closing all windows and doors the instant the medical officers left the wards, with the view, as they stated, to obviate draughts, and thus practically converting the institution into a pest house. At a subsequent period, all other means having failed to do more than mitigate the evil, on the suggestion of the master, Dr. Clarke, a considerable number of holes were cut in all the doors and window frames, and thus the admission or exclusion of fresh air was placed beyond the control of any; the results were, that the malady ceased its ravages, that infantile mortality was greatly diminished, and that a case of *trismus nascentium* is

now but seldom met with. Dr. Clarke was the first master who delivered courses of lectures to the pupils of the hospital, although, as already stated, a lectureship existed since 1774.

Dr. John Breen, following in the footsteps of Dr. Clarke, compiled the statistics of the same institution during his three years' Assistantship. He also published a short treatise on *Trismus Nascentium*, recommending for its treatment minute doses of opium. Throughout a long life he maintained an honourable name in his profession, which he pursued to almost the latest period of his life, and 'tis but a few years since, at the age of more than eighty, he sank into his honoured rest, full of years and good repute.

Dr. Samuel Bell Labatt—sometime Master of the Dublin Lying-in Hospital—was another of those who reflected no little credit on Irish midwifery practitioners. His treatises on "Ruptured Vagina," "Prolapse of the Uterus," and on "Vaccination," have added considerable value to our literature ; and it was only two or three years before his death, when far advanced in years, that he feared not to stand forth, the champion of his own views on the subject of re-vaccination, although his opponent was younger, more vigorous, and truly formidable—the late Professor Montgomery. Dr. Labatt has left behind him a monument which redounds infinitely to his credit as a philanthropist, and to his reputation as a physician, namely, the Cow Pock Institution. His attention having been directed to Jenner's views, Dr. Labatt saw at a glance the importance of establishing vaccination as *a system* in Ireland ; he had, however, much difficulty to contend with, especially among the lower classes, whose prejudices against it were of the strongest character ; but convinced of the soundness of Jenner's reasoning, and determined to break down all opposition, he enlisted the sympathies of the higher ranks of society, he procured and brought to bear the aid and influence of the clergy of all denominations, and, in fine, left no means untried to accomplish his favourite object, until eventually prejudice yielded to reason, and he lived long to witness the successful issue of his exertions. He was appointed Secretary to the Cow Pock Institution immediately on its foundation, and retained that office up to the period of his death ; and it was in a great degree owing to his exertions that an annual Government grant was accorded to its support. I may here add, parenthetically, that the service rendered by this institution in the cause of humanity is beyond calculation, the more especially since the application of the Compulsory Vaccination Act to Ireland, as it is the head quarters and source of supply of healthy lymph to all districts of the country.

It is but a few brief years since there passed from amongst us one whose simple-minded, yet high-toned character, redounded no little credit on his profession. After a long life of active practice, Dr. John

Douglass died as he had lived respected and esteemed. During his three years Assistantship in the Dublin Lying-in Hospital, he eliminated his theory regarding "The Spontaneous Evolution of the Fetus," in presentation of the upper extremity. The brochure, wherein he gave his views to the profession, proved indisputably the correctness of his observation, and the soundness of his reasoning, whilst it, at the same time, overturned the opinions of all preceding authors on this interesting subject. A second edition of this valuable treatise was published shortly before his death, a copy of which he presented me with on the occasion of my last interview with him only a few days before his demise. Dr. Douglass enjoyed the questionable notoriety of having been the only metropolitan practitioner of that period who escaped the withering venom of Dr. Brennan's gall-tipped pen.

We would scarcely expect to find the name of Dr. John Kirby—whose position at the front of the Irish surgeons of his day is too well known to require comment from me—commingled with obstetric practice. Although, like Mr. Dease, he turned his exclusive attention to surgery during the latter years of his life, yet like him, he, for many of the earlier years of his professional career, practised midwifery, and although he never wrote on any topic connected with that branch of his profession, yet he attained such eminence therein, that he was looked up to as one of its consultants, whose advice was sought for by his junior and less experienced practitioners, in cases of extremity. He had for some time contemplated giving up that branch of his profession, when he was confirmed in his determination by the following circumstance, the particulars of which I learned from himself:—A case of extreme difficulty having arisen in the practice of one of his brethren, Dr. Kirby's advice as a consultant was sought for. From great deformity of the bony structures, they both agreed that the patient could not deliver herself—that even with the most judicious assistance she could never give birth to a living child—that lessening the head of the child was the only means of safety to the mother—that if this was not done both mother and child must inevitably be lost, and that they could lose no time in carrying out their decision, as the patient had already begun to sink. The operation was rapidly performed by the first attendant, and they were both still engaged with their patient, when the wretched infant—who had not been altogether deprived of life by the operation, although the greater portion of the brain had been of necessity broken up—horrified all who were assembled in the room, by suddenly and loudly crying out. Immediately the original attendant adopted the more than questionable course of placing the heel of his boot on the head of the child, and crushing it down. A horrid whisper at once passed round amongst the lookers on; and whilst a coroner's jury exonerated the immediate actor from all responsibility, yet still public opinion ran counter to him, and from that time he completely lost

his midwifery practice. So horrified was Dr. Kirby at the occurrence, that he determined at once to carry out the intention he had already formed, and he never again attended a case of midwifery.

Contemporaneous with all the foregoing practitioners of the early part of the nineteenth century was one to whom I have already referred on more than one occasion—the talented but unpopular Dr. John Brennan, commonly known as the "wrestling turpentine doctor," but designated by himself, "Prince of Idonah, king of all the wrestlers of Ireland." This extraordinary man was possessed of an almost inordinate degree of racy wit, but combined with scorching sarcasm and overwhelming drollery. He spared none; and yet even those at whom he cast his keenest shafts. while they writhed under the infliction, could not help being amused when time had healed the envenomed wound. The *Milesian Magazine*, of which he was the editor, and which was the vehicle of his satires, ran to sixteen numbers, published at irregular intervals between the years 1812 and 1825. He survived the publication of the last number but five years, having died in Dublin on the 29th of July, 1830. It is much to be regretted that a man of his high classical attainments and great talents should have turned his rare powers to an unworthy use. But we must not suppose his life to have been utterly valueless. Hitherto he has been referred to merely as a clever satirist, whose hand was against every man and against whom was every man's hand. I must now change the picture, and hold him up to your view as one of the greatest benefactors of his race. To him are we indebted for the general use, both internally and externally, of turpentine in the treatment of puerperal fever; and hence his *soubriquet* "Turpentine Brennan." This was his hobby; and, like all others who get mounted on such a Pegasus, he rode it a little too hard. Whilst, then, I cannot go the length of admitting that turpentine will cure, as he believed it would, every case, sure am I that its early employment will, in very many cases, put a stop to symptoms which, if not thus terminated, would speedily run into that frightful malady; and moreover, that it is a valuable adjunct to other remedies in even its later stages. When he first propounded his views in relation to this remedy he was met with ridicule, and subsequently with opprobrium; yet still he persevered, merely revenging himself in his lampoons, until at length the current of public opinion ran in its favour, and the value of the remedy was, to a certain extent, admitted. In fine, I have no hesitation in asserting that it is altogether impossible to estimate the number of lives saved through its instrumentality; and that, whatever were his failings, to Dr. John Brennan is due a deep debt of gratitude for his invaluable suggestion, and that his genius deserves our admiration and commendation.

Within my period of office as your President, there has passed away

from amongst us one of the most estimable of our brethren, deservedly beloved whilst he was with us, and deeply mourned, now that he has been taken to his rest. Dr. Robert Collins became Master of the Dublin Lying-in Hospital about the year 1825, and during his seven years' tenure of office devoted himself to the minute study of practical midwifery; and in 1836, shortly after the expiration of his term of office, he published his experience in a work on Practical Midwifery, which gave the result of 16,414 cases. Whilst the value of this publication, in a statistical as well as practical point of view, can be scarcely overestimated, yet a few of his conclusions were questioned by some, and among others, by Dr. (afterwards Sir) James Simpson, the distinguished Professor of Midwifery at Edinburgh, who carried on a lengthened correspondence with Dr. Collins on the topics in dispute. At the termination of the encounter neither had made a convert of the other, but they each conceived, as is usual on all like occasions, that he himself had come off victorious. Dr. Collins enjoyed most extensive private practice for many years. As a physician he was highly gifted; while, as a consultant, his brethren looked upon him as one of the soundest, safest, most discreet, and high-minded in the profession; and on points of professional ethics or etiquette there were none whose opinion had greater weight. He was for two years—1847 and 1848—elected President of the King and Queen's College of Physicians, being the first *exclusively* obstetric practitioner who was appointed to that important office. For some years prior to his demise he had retired from practice, and resided on a property at Navan, which he had purchased. He frequently visited his friends in Dublin, with whom he was always a welcome guest; and I may add that, to the last moment of his life, he enjoyed the good-will and kindly feeling of all with whom he was at any time brought in contact.

I have already stated that Dr. William Fetherstone Montgomery was elected Professor of Midwifery by the King and Queen's College of Physicians in 1827. When they awakened from the lethargy which I have already depicted, and became convinced that obstetrics constituted an essential element in the education of every medical man, they looked about for a professor suitable for the chair, and never did their choice fall on one more fitted for it. Professor Montgomery was not merely thoroughly conversant with both the theory and practice of his subject, but was likewise a clear, concise and pleasing lecturer, and in these respects was unrivalled among his compeers; while, at the same time, the classic beauty of his language, combined with his elegant diction, had the effect of almost fascinating his auditory. He had, moreover, the rare but agreeable faculty of divesting his subject of all technicalities when addressing a mixed assembly, and making himself intelligible to the least informed, thereby precluding the possibility of any feeling of

tedium. His resignation of the professorship was deeply deplored by the profession in Dublin, and was most unwillingly accepted by the College, of which he had been President for two years. His "*Signs and Symptoms of Pregnancy*" is a perfect medical classic, and has become a library book eagerly sought after, and in the possession of every member of his profession who can obtain it; and has raised for its author a deservedly high renown, which his many contributions to the Journals has in nowise lessened.

The year 1829 introduced into practical midwifery a new era, and that, too, through the medium of the medical school of Dublin. On the 5th of October in that year, Dr. John Beatty—need I add, the father of the deservedly popular member and lately President of this Society—submitted a paper to the Association of the College of Physicians on "The Use of Instruments in Cases of Difficult and Protracted Labour." Antecedent to this date the employment of the forceps had fallen much into disuse, and the perforator and crotchet substituted, more especially in first cases, wherein the former instrument was utterly discountenanced; but the practical mind of Dr. John Beatty saw the absurdity—nay, the cruelty and criminality—of pursuing such a course; and, casting aside all his preconceived notions, he investigated the subject without prejudice, and tested to the utmost his newly-formed opinions. His views having been fully confirmed by his experience, he demonstrated, in the paper I have named, that the forceps should be restored to the position they at one time held; that they were perfectly safe and easy of application; and that they were suitable for employment in even first cases—a heresy which brought on him no small amount of obloquy from those who condemn everything that is new, merely because it is new. He, moreover, unhesitatingly denounced the use of the perforator and crotchet, unless in exceptional cases. "In conclusion," he says, "I will observe that nothing short of the most imperative necessity can warrant the use of the destructive instruments; and no case can be considered as demanding them until every means by which both mother and child might be saved have been put into requisition and fairly tried." These noble sentiments, and this sound practice, after a time swept away the opposition at first raised against them; it was not, however, for some years that all prejudice was removed; but now, thanks to the talent and courage of this great reformer, common sense has gained the ascendancy, and the practice of Dr. John Beatty in this respect has been adopted throughout the civilized world. I need scarcely remind you that very many contributions—too numerous to specify—to the medical Societies and Journals of his time emanated from the versatile pen of this practical physician; but his letter to the Association of the College of Physicians, read at the December meeting, 1821, on "A Species of Premature Labour," deserves attention

from the originality of thought and acute observation manifested in it. In this paper he demonstrated that not merely premature labour, but also the death of the fetus in utero, is the result of venereal poison in the system of the parents, and that the remedy is mercury.

I cannot here omit to mention a cotemporary of many of the worthies I have hitherto referred to, and who was with us up to a few years back, and honoured by us to the last moment of his existence.

In 1815 Dr. Charles Johnson obtained the license of the Royal College of Surgeons, and in the February of 1816 became assistant to Dr. Labatt, the then Master of the Lying-in Hospital. In 1818 he was elected a member—analogous to the present Fellow—of the Royal College of Surgeons. Having devoted himself, in consequence of a defect in vision, to the practice of obstetrics and the diseases of women and children, he, in the year 1822, in conjunction with Sir Henry Marsh, established the "Pitt-street Institution for the Diseases of Children," the first institution founded for treating the accidents and diseases peculiar to early life. So high a reputation had Dr. Johnson acquired in the respective branches of his profession which he practised, that on his application for the Professorship of Midwifery in the Royal College of Surgeons in 1828, he was the successful candidate. In 1829 he obtained the licence of the King and Queen's College of Physicians; in November, 1840, he was elected Master of the Dublin Lying-in Hospital; and in 1841 obtained the high but well-merited distinction of the Honorary Fellowship of the College of Physicians. The course invariably pursued in his private practice by Dr. Johnson—insisting on the nomination of his own nursetender in every case—so completely commending itself to our notice and consideration as materially tending to the well-being and complete recovery of the patient, and to the reputation of her attendant, that I feel I cannot too strongly impress its adoption on all obstetric practitioners. Dr. M'Clintock, writing on this point, says:—"From his first starting in practice Johnson was most particular in the selection of his nursetenders, and would not permit his patients to employ any he did not himself approve of. This was then a rather novel course to adopt, and was often stoutly opposed by the ladies, who considered they ought to have as full liberty in the choice of their monthly nurse as of any domestic servant. But Johnson steadily adhered to the rule he had laid down. By so doing he was able to keep a very wholesome and needful check over this class of medical subordinates; and we, accouchers of the present day, are much indebted to him for possessing an undisputed right of nominating the nurse, without which we should have no real control or authority over her or the patient." Irrespective of this point, he, in all other respects, exercised the most arbitrary authority over his patients, demanding from them the most implicit obedience to his behests; and so much was he respected, and so highly were his

opinions regarded, that he but rarely met with a deviation from his instructions.

Dr. Johnson, although one of the School of Practical Accoucheurs, has not it is to be regretted, published the result of his experience whilst Master of the Dublin Lying-in Hospital, but he has given to the world an admirable treatise on "Hooping Cough." in the Cyclopedia of *Practical Medicine*, and an essay on the "Removal of the Inverted Uterus." It is much to be regretted that one so capable should not have added more to our midwifery literature. He died June 19th, 1866.

I now come to another event of no inconsiderable importance in the history of midwifery progress in Ireland—namely, the founding of the Coombe Lying-in Hospital. Prior to the year 1826 there did not exist any institution on the south side of the metropolis for the relief of indigent lying-in women. The distance of the Dublin Lying-in Hospital was so great from the south-western district that hundreds preferred remaining at home to venturing, amidst the pains of labour, on a considerable journey, too frequently to be performed on foot, if attempted at all; and consequently confided themselves, in all probability, to some one or other of the many so-called midwives, who were, however, in most instances, merely "lucky women," as they were designated, but whose ignorance was only exceeded by their audacity—a class which abounded in the specified locality. The result is easily told; many of them never left their beds of suffering until they were carried forth to burial, the victims of the grossest ignorance. Others, however, made the attempt to reach the distant asylum at the Rotunda, but some failed to accomplish their object; and it is recorded that during the severe winter which preceded the opening of the Coombe Lying-in Hospital, more than one poor woman, taken with the throes of labour on her way, perished with her offspring in the snow. It will be recollected that in the year 1826 the Governors of the Meath Hospital had removed to their new and spacious buildings in Long-lane. Their vacated premises on the Coombe were at once purchased and maintained as a surgical hospital, with an extern midwifery dispensary attached, under the care and supervision of Dr. Richard Gregory. After the lapse of three years the surgical department was abandoned; but through the exertions of Dr. Gregory the establishment was, in 1829, converted into a lying-in hospital, under its present name. Its founder could not, like Mosse, claim any originality in the idea, but he brought to bear upon the task he had undertaken considerable talent, great zeal, and indomitable industry. Short, however, was his brilliant career; he survived the achievement of his object only two or three years, cut off in the prime of life by a malignant fever acquired in the discharge of his duty. In the arrangement of the hospital he fitted up thirty beds, the largest number he anticipated funds — which were derived exclusively from private

benevolence—to maintain; but feeling that the relief thus afforded would not be at all commensurate to the wants of the locality, and knowing, from his three years' previous experience, that many poor women are unwilling to go into hospital, and thus leave their homes and families to the precarious care of strangers, he continued the economic and admirable system carried out from 1826 to 1829—of giving attendance from the hospital on labour cases at their own homes; the combination of intern reception with extern attendance has consequently continued to be, ever since its foundation in 1829, a distinctive characteristic of the institution.

For ten years subsequent to its opening as an hospital all its beds were devoted to the exclusive reception of labour cases. In the year 1839, however, the want of a ward for the treatment of the diseases peculiar to females having been previously much felt, Mr. Hugh Carmichael, the then Master of the hospital, following the example set by Dr. Evory Kennedy in the Dublin Lying-in Hospital, appropriated seven beds exclusively for this class of cases—an arrangement which has tended materially to the interests of the sick female poor of the locality in which the institution is situated, and of the students attending its practice.

I have already referred to the withdrawal of the parliamentary grant from the Dublin Lying-in Hospital, and its subsequent restoration, on however, a very reduced scale. It will bein the recollection of most of those whom I now address that for many years the grants to all the Dublin Hospitals which had been previously in receipt of such, were being gradually withdrawn, with a view to their final extinction, and that, too, despite all the influence which could be brought to bear on the Government of the day. An agitation on the subject was, however, got up, and a Committee of the House of Commons, which had been most reluctantly appointed, reported in favour of the restoration of the grants. In consequence of the recommendations contained in that report, a Government Commission was appointed to investigate the whole subject; and under their advice the whole amount of the original grants was restored, but with slight alteration in the allotment. Under the arrangement thus made £200 per annum was granted to the Coombe Lying-in Hospital, which had never previously enjoyed the benefit of a Parliamentary grant, nor the *prestige* of a Government hospital.

Despite, however, the general prosperity of the institution, adequate funds being at this time provided through the medium of private contributions, a city presentment of liberal amount, and the annual grant by Parliament already referred to, yet one thing was still wanted to secure for it a suitable status, namely, a Royal Charter of Incorporation; this was conceded on the recommendation of the Duke of Abercorn, in 1867, when Her Majesty's letters patent, under the great seal, were issued to the institution.

That the accommodation afforded by the hospital was altogether inadequate for its intended purpose was for many years manifest, and its suitable enlargement contemplated; this, at length, is being effected by the erection of a large wing, wherein provision is made for a far more considerable number of cases of the diseases of females, than the limited capabilities of the existing building afforded; in it, too, will be a ward allocated for the treatment of the diseases of children, and a ward, approached by a stair, devoted altogether thereto, will be set apart for the exclusive reception of such cases of infectious diseases as may arise in the lying-in wards of the hospital itself.

So soon as this wing shall have attained completion, the entire of the original building will be devoted exclusively to the reception of labour cases; in a few months, too, a considerable enlargement of this building will be commenced, whereby the accommodation for obstetric patients will be more than doubled, and thus the institution in its entirety will be provided with more than eighty beds, and will have attained a size approximating, but still infinitely less than the noble proportions of its northern sister. I feel assured that every one who now hears me will echo the aspiration I now express, that the harmony which now exists, and has so longprevailed between these two kindred institutions, both devoted to the same great cause, may ever continue, and that the only rivalry which may be ever developed between their respective medical staffs, shall be the laudible emulation of each other in the cause of humanity, in the advancement of science, and in the promotion of the honour of their noble profession.

In alluding to the intended establishment of a children's ward in connexion with the Coombe Lying-in Hospital, it is only right to state that the idea is by no means original. Dr. Evory Kennedy, during his Mastership of the Dublin Lying-in Hospital, had hoped to have opened such a ward therein, but was unable to fulfil his intention; other institutions—both medical and surgical—have, since then, most laudibly established wards for the treatment of the diseases of children; but I agree with Dr. Kennedy in thinking that a Lying-in Hospital is, properly speaking, a suitable habitat of a children's ward, and that no lying-in hospital can he considered complete without one.

A very great stimulus was given to the scientific study of midwifery, by the establishment of the Dublin Obstetrical Society in 1838. I need not here, in presence of that Society, recapitulate the circumstances which brought it into existence; suffice it to say that Dr. Evory Kennedy, then Master of the Dublin Lying-in Hospital, was its founder, and that Sir William Wilde, Professor Richard Doherty, of Galway, and myself—all then students attending the practice of that hospital—were its original members. Perfection, if ever attained, is never acquired at once, and so too has it been with this Society, wherein, certainly, many alterations

have taken place from time to time, and whereon another generation will probably pass a verdict.

At first this Society was constituted of students—chiefly those attending the practice of the Dublin Lying-in Hospital—who met in an apartment in the Rotunda buildings, adjoining the hospital, and were presided over by some one or other of the eminent obstetricians who were then, or had been, connected with that institution. In fact, the Society might have been at this time styled, the Dublin Lying-in Hospital Obstetrical Society, rather than the Dublin Obstetrical Society; but it fulfilled its mission so well in even that limited sphere, that others began to desire membership, and after the lapse of years, a marvellous change has passed over the constitution of the Society, which has now, for a lengthened period, opened its arms to all eligible for admission, no matter whence they come; the most eminent of the profession now aspire to its membership, and take part at its meetings; and it has become, what it ought to be, metropolitan in its character; whilst, as it were, to impress it with the stamp of professional approbation, the King and Queen's College of Physicians of Ireland have afforded the Society a local habitation, by placing the magnificent hall, in which we are now assembled, at its disposal for its meetings.

With such a change in its constitution it might be expected that a corresponding improvement would take place in its efficiency, and that such has been the case is demonstrated by the very enlarged roll of its members, by the considerably greater attendance at its respective meetings, and especially by a reference to its "Proceedings"—the value of some of which can scarcely be over estimated—published from time to time in the *Dublin Quarterly Journal of Medical Science*, the authorized organ and exponent of the Society.

In accordance with usage, I now submit a list of the papers presented to the consideration of the Society during my year of office, and I would pray your permission to prefix to it the titles of the papers read at the respective meetings held in the course of the preceding year, the untimely death of my lamented predecessor in this chair—Dr. Hardy—near the close of his term of office, having hitherto precluded any such return for that period.

Dr. M'Clintock read a paper on the *Spontaneous Elimination of Uterine Tumours.—December 14th*, 1867.

Mr. Tate of Wakefield submitted a paper on *Chorea.—January 25th*, 1868.

Dr. Byrne a case of *Cephalotripsy*, and exhibited *a cast of the head taken before the Cephalotribe had been removed, and a Section of the Skull.—January 25th*, 1868.

Dr. Speedy read a case of *Inversion of the Uterus.—February 8th*, 1868.

Dr. Moore read a case of *Hydatidoid Ovum*, and exhibited *a portion of the Specimen.—February 8th*, 1868.

Dr. Kidd read a case of *Prolapse of the Funis successfully treated by the Postular Method.—February 8th*, 1868.

Dr. Henry Kennedy read a paper on the *Treatment of Sleeplessness in the acute diseases of Childhood.—February 8th*, 1868.

Dr. Charles F. Moore exhibited an *Aborted Ovum of about six weeks, in which a large effusion of blood had occurred between the chorion and amnion. —March 4th*, 1868.

Dr. Byrne read a paper on *Retained Menses from imperforate hymen.— March 4th*, 1868.

Dr. Halahan read a paper on *Anomalies in the Mechanism of Parturition. —March 4th*, 1868.

Dr. J. Guinness Beatty read a case of *Rigid Perineum*, which he had divided as suggested by Dr. Thomas Beatty.—*March 4th*, 1868.

Dr. Thomas Beatty read a case of *Fibro-cystic Tumour of the Vagina removed by Ligature*, and exhibited the tumour.—*April 1st*, 1868.

Mr. Harley read a case of *Gonorrheal Rheumatism after Parturition.— April 1st*, 1868.

Professor Macdonald of St. Andrews read a communication on the *Fetal Circulation*, in which he propounded certain novel views.—*April 1st*, 1868.

Professor Hurley forwarded a letter enclosing a *photograph of a boy now in his sixteenth year, whom he had removed from the womb of his mother by Cesarean Section after her death from disease of the heart.—April 1st*, 1868.

Dr. Woods of Roscrea made a communication in reference to two children of doubtful sex, and subsequently exhibited the children.—*May 7th*, 1868.

Dr. J. Guinness Beatty exhibited a recent *Hydatidoid Ovum*, and detailed the case.—*May 7th*, 1868.

Dr. Kidd exhibited *two uteri containing polypi and fibroid tumours*, and read several cases of *intra-uterine polypi;* and described a *modification of the ordinary method of exploring the uterus.—May 7th*, 1868.

Dr. Ringland read a paper on *Version in Contracted Pelvis.—June 20th*, 1868.

Dr. Hardy—the President—read a paper on *Scarlatina in Pregnancy and the Puerperal State.—June 20th*, 1868.

From this it will be seen that the last presidential act of Dr. Hardy was to add to our *repertoire* a most valuable communication. On the re-assembling of the Society, at the commencement of the next session on November 21st, the first communication submitted was a memoir, by his friend Dr. M'Clintock, of our late highly esteemed President who had meanwhile passed from amongst us.

Dr. Atthill read a paper on *Retroflection of the Uterus.—Dec.* 12, 1868.

Dr. Kidd exhibited *fifty-five varieties of Pessaries.—December 12th*, 1868.

Dr. Thomas Beatty exhibited *a Pessary of his own invention.—December 12th*, 1868.

Dr. Morgan exhibited *drawings and specimens showing a particular form of Syphilitic Ulcer of Female Organs of Generation.—February 13th*, 1869.

Dr. M'Sweeny showed two tumours removed from a placenta, which proved to be encysted blood clots.—*February 13th*, 1869.

Dr. M'Clintock read a case of *Labour subsequent to the cure of a large Vesico-Vaginal Fistula without injury to the Cicatrix.—February 13th*, 1869.

Dr. Churchill exhibited Dr. Mathew Duncan's Cephalotribe.—*February 13th*, 1869.

Dr. Kidd exhibited his *Cephalotribe with plaster casts of two heads operated on with it.—February 13th*, 1869.

Dr. Evory Kennedy read the first part of a paper on *Zymotic Disease.—March 13th*, 1869.

Dr. Sawyer made a comunication on *Atresia of the Vagina.—March 13th*, 1869.

Dr. Evory Kennedy read the second part of a paper on *Zymotic Disease.—April 9th*, 1869.

The variety and versatility of the several papers detailed in the foregoing lists require no comment from me, the very names of their respective authors being ample guarantee for the practical manner in which the individual subjects were handled. I cannot, however, omit to mention that of such vast importance was the subject of Dr. Evory Kennedy's communications on Zymotic Disease deemed to be, and of such magnitude were the points at issue, that they occupied the attention of the Society for eight nights—six special meetings having been devoted to their discussion, and engaged the advocacy on either side of the question of most of the ablest practical Obstetricians this metropolis contains—twelve of the speakers being arrayed against and three in favour of the author's views. This very divided opinion to my mind clearly demonstrates the fact, that whatever the result, the subject not merely merited but demanded discussion.

I must now for a brief space recall your attention from institutions, to individuals. Amidst the horrors of the Indian mutiny, General Nicholson, the gallant leader of the heroic little corps that assailed the foul band of murderers who held the iron bound walls of Delhi, received a mortal wound, and in a few days this brilliant soldier was numbered with the dead. Many an eye drooped, and many a tear was shed to the memory of that brave man, but few, perhaps, knew that he was the son of a Dublin obstetrician of considerable practice and high repute, who died in the prime of life about five and thirty years ago, leaving behind him an unsullied reputation, and a representative whose name now belongs to the pages of the history of his country.

The late Dr. Robert Johns never published his experience save in the form of communications to the Medical Journals. One of the most inter-

esting of these, on the rapid blistering of the os uteri by means of vesicating collodion, attracted much attention at the time. He was removed hence while still in the prime of life, and gradually acquiring public confidence.

The admirable and interesting memoir of the late Dr. Samuel Little Hardy so recently submitted to the Society by Dr. M'Clintock has left me little to say regarding him; besides the great length to which this address has extended from small beginnings reminds me that I must hasten on. The valuable statistical, and practical results of their three years "assistantship" in the Dublin Lying-in Hospital published by him and Dr. M'Clintock conjointly, have raised a reputation for these authors of which I would speak more fully, were it not for the rule laid down by me at the outset which precludes me doing more than merely referring to any living celebrity; and it would be impossible to do justice to one, without, by implication at least, mixing up with it the public estimate of the other. Dr. Hardy also supplied the journals with many practical communications, and earned for himself a just celebrity by his invention of "The Chloroform Douche," an apparatus for the local application of chloroform, and which, although not now in use, certainly led the way for the introduction of Richardson's spray apparatus for local anesthesia.

I have now completed my *annals*, and have limited my statements, according to my expressed intention, to the records of the dead and of times gone by, and I trust that I have dealt faithfully and impartially with their memories, whilst, at the same time, I have not trod too heavily on their graves:—that whilst I have not grudged a "little dust of praise," I have "in nought detracted, nor aught set down in malice:" I have endeavoured to shadow forth, faintly, it is true, but I hope faithfully, the glorious band who pioneered the way to the proud position the midwifery branch of the profession at present holds, which, I have no doubt, the many able, energetic, and high-minded men who are still amongst us will not merely maintain but will materially add to.

There is one point wherein I conceive the present generation can do something tending somewhat to this result, and which I would venture here to feebly shadow forth. Medicine held no status as a profession until its professors were brought together as a college; surgery was but a trade—allied and even subordinate to one of the meanest occupations, until after the incorporation of its followers into an united body. 'Tis true that midwifery does not now want any extraneous aids to its position, but I maintain that its status can be confirmed and placed beyond the possibility of retrogression by the united action of its professors. Why, then, I would ask, should not obstetricians be amalgamated together in an incorporated society for their common weal and common advancement? Why should midwifery be a mere offshoot of any other bodies—no matter how high their prestige, or how respectable their character—instead of an independent corporation? Why should there not be a Royal College of

Obstetricians as well as of Physicians or Surgeons? My views on this point may appear Utopian to many, but I believe the germs of such a college do exist and could be found, and that if but a few of those who are now assembled in this hall, would take the matter in hand, and would apply to the task a small portion of the energy they possess, and which they have proved in other matters, the founding of such a college would speedily be *un fait accompli.*

And now, Gentlemen, I have done; my task is accomplished; how imperfectly it is for you to judge. Nothing further remains for me but to offer to you my sincere thanks, which I do most fervently, for the great honour you have paid me in electing me your President, for the forbearance you have shown to my many shortcomings and failings during my year of office, and for the kindness and patience with which you have listened to this somewhat lengthy and tedious address.

Case of Polypus Uteri—Hemorrhage—Removal, followed by Pregnancy.—Dr. John A. Byrne said that at a previous meeting of the Society he had exhibited a large fibrous polypus which he had removed from the uterus of a woman unmarried, and near the menopause. Before its removal it had given rise to long-continued and alarming hemorrhage, which ceased upon the removal of it.

The polypus which he now took the liberty of presenting to the Society was one which he had removed some time since from the uterus of a patient; and he would, in the most brief manner, relate its history.

A married woman, aged thirty-five, consulted him on account of metrorrhagia, from which she had been suffering for nearly one year. She was perfectly anemic, was feeble, and, in fact, presented all the appearances of one who had been losing blood for a considerable time; there was scarcely any interval of time between the discharges of blood; in fact, it had been one continuous drain since her second marriage, which took place about one year before she consulted me. She had been married before, had two children; but during her widowhood had never suffered from hemorrhage.

On making a vaginal examination I felt this tumour floating about loosely at the upper part of the vagina, and attached to the anterior and upper part of the cervical uterine canal.

I explained to her the nature of the affection, and she willingly consented to its removal.

Dr. Kidd and Mr. Tyrrell were present and gave me their assistance. A vulsellum was fixed in the tumour, and a small ecraseur being introduced, the wire was slipped around its pedicle and it was removed. There was no hemorrhage, and she quickly recovered.

Dr. Byrne exhibited the polypus, which was the size of a walnut.

In about nine months afterwards I saw her; she was then four months

pregnant, and had enjoyed the most excellent health ever since the removal of the polypus.

The case is interesting as proving that not only can a polypus growing from the cervix uteri be the cause of long-continued and severe hemorrhage, but also that the fact of its presence is a preventative to the occurrence of conception, which circumstances, however, have been long known to obstetricians. However, I think that every additional case of this kind to our knowledge will be deemed important by our Society.

Case of Atresia Vulvæ from Sloughing during Fever—Operation—Pregnancy.—Dr. John A. Byrne said:—At our meeting of 13th February last we were indebted to Dr. Sawyer for a most interesting account of a case of that exceedingly rare congenital malformation—atresia vaginæ—and for the exhibition of the organs themselves. The case, too, had a most important bearing in this respect, that it had been closely watched by him for a very considerable period of the patient's lifetime, and that he was made familiar, from the varying symptoms which occurred during life, of the gradual subsidence of the menstrual functions; and the diagnosis made by him, and other medical men who saw the patient, was verified in the most remarkable manner by the examination of the organs after the death of the patient.

The case which I bring before the notice of the Society, although not presenting so many features of interest, is, however, not devoid of it, as it illustrates the importance of an examination of the sexual organs in cases where malformation exists. It is a case of labial or vulval atresia, produced by the adhesion of the inner surfaces of the labia from sloughing, after fever, and which was remedied by simple separation of the opposite surfaces.

M. Kearns, aged twenty, consulted me in July, 1867. She stated that she wished me to examine her on account of there being something astray, as she stated, in her external parts of generation. Having placed her in the proper position, I found the following to be the case:—The body was well developed; the mons veneris also; there was rather a scanty supply of hairs upon the pubis. On separating the thighs widely, so as to examine the sexual organs, I perceived that the two labia majora were united in their entire length, and were very much atrophied; there was very little hair upon them, and there was no appearance of labia minora nor clitoris. The uniting medium of the two labia was of very dense consistence, and drew them very closely together.

At the upper part of the vulva there was a small aperture, sufficient to admit the point of a sound. Through this the urine and menstrual fluid came. When a probe was introduced into this opening it was impossible to say whether it went into the bladder or not.

The whole appearance of the genital fissure was very extraordinary, and very different from that of the normal condition.

She stated that her general condition was that of good health; that she made water regularly; she had the usual sexual feelings; and, in fact, was about being married.

Like a prudent woman, she wished to have all obstacles removed previous to such an important event.

The account she gave of herself was, that when a girl of about eight years old she had had fever, and that the parts had become adherent after her recovery.

I explained to her the necessity of removing the impediment which existed; and my friend Surgeon Tyrrell admitted her into one of his wards in the Mater Misericordiæ Hospital.

There was very little difficulty in the operation. An incision was made into the centre of the labial plane; and a director being passed upwards and downwards, this incision was enlarged, and the vagina was communicated with. We then found that the clitoris, meatus urinarus, vestibule, and labia minora were in their proper place; and although rather less developed than perhaps they would have been under an opposite condition of things, still it could not be said that atrophy existed. I passed my finger up the vagina and could feel the cervix uteri and os. A plug was kept in the opening for some time, and no bad effects followed.

She married soon after leaving the hospital.

I had an opportunity of making an examination some time afterwards. She was in the eighth month of pregnancy.

In looking at the genital fissure it would scarcely be supposed that any contraction or closure of it had ever existed. The ostium vaginæ large; the labia were developed; they and the pubis were more largely supplied with hairs, and altogether the organs had very much the natural appearance.

She stated that she suffered very much from intercourse shortly after marriage, but that now there is no pain whatever.[a]

I am not aware of there being many cases on record of labial atresia from such a cause; we know, however, that sloughing of the genital passages occasionally takes place in severe cases of scarlatina. Noma pudendi sometimes, too, takes place; and perhaps, may lead to this affection, and call for interference in after life should the patient recover.

Congenital labial adhesion sometimes occurs; and I thought at first that this was a case of this nature. The girl, however, was positive that such a malformation did not originally exist, as her mother had been careful about her, and her sister remembered the time of the occurrence.

[a] Since the above was written, she has been confined in the Rotunda Lying-in Hospital; and it was necessary to incise the perineum during the last stage of labour, Dr. Madden, the assistant, having found it so resisting as to be an obstacle to the exit of the child's head.

In the first vol. of Bernutz and Goupil, there is a case detailed where menstrual retention took place in consequence of a cartilaginous obliteration of the vagina. She never menstruated, although constitutional symptoms of menstruation showed themselves. An accumulation took place interiorly, which terminated in rupture of the Fallopian tubes, and the death of the patient.

Had this malformation been detected, and the septum incised, exit would have been made for the fluid, and this fatal result would not have taken place.

Case XIV.—They also give the history of another case in which sloughing took place from typhoid fever, and the formation of bands which were removed by operation. This case also terminated fatally.

Congenital labial adhesion is taken notice of by Scanzoni, who says that it is rarely complete. Should it not be recognized at birth, if complete, the infant will die from the impossibility of micturition. He has observed two cases; in both there was an opening at the upper part, which allowed the urine and menstrual fluid to pass.

One of the patients, he says, was married, and consulted him after three years. He operated in a similar way to my case, and in ten months the woman proved pregnant. He does not mention whether intercourse had had any effect upon the septum; but it would appear that the case is very like that which I have read to the Society, differing only in the fact that in my case, according to the patient's account, the malformation was the result of gangrene; in his case it was a congenital malformation.

M. Raciborsky, one of the latest authorities upon the disorders of menstruation, says (page 529):—

"Il y a aussi des atrésies vaginales accidentelles, relativment encore plus graves que celles dont nous venons de parler, a cause des irrégularitès dans les adhérences des parois opposes et á cause de la nature du tissu cicatriciel. Elles sont généralement la conséquence des inflammations violentes de la muqueuse vaginale, occasionnès par des injections irritantes pratiquèes mal a propos, par des cautérizations trop profondes ou la gangrène de la membrane muqueuse. Plusieurs auteurs rapportent des examples d'atrésies de cette espèce, a la suite de gangrène dans la cours des fiévres typhoides, du choléra, dans la scarlatine, la variole, etc. Scanzoni compte même la diphthérie parmiles causes des atrésies vaginales accidentelles, dont il est question."

Within the last three months I have met with a very remarkable case of congenital atresia vaginæ. An unmarried woman consulted me, and told me she had never menstruated. She was very well formed; the mammæ were normal. In every respect she appeared well developed. She allowed me to examine her, and I could find only the meatus urinarius and anus, and between them a small pouch, just sufficient to admit the

top of the finger. On examination per rectum I could not feel any uterus, and the sound, when introduced into the bladder, could be felt most readily by the finger introduced into the rectum, a thin septum only intervening. No hard substance corresponding to the uterus was perceptible to the finger on the most careful examination. The mons veneris and labia were sufficiently large; and there were hairs upon the pubis. She had sexual feelings when younger, and suffered from pain occasionally; but there never was any discharge from either of the other orifices, nor vicarious discharge. I have no doubt this is a case of congenital absence of the internal organs. I have the case still under observation, and may, perhaps, at some future time be able to say more about it.

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2. Failure of sight from railway and other injuries of the spine and head, its nature and treatment, with a physiological and pathological disquisition into the influence of the vaso-motor nerves on the circulation of the blood in the extreme vessels. By Thomas Wharton Jones, F.R.S., F.R.C.S., professor of ophthalmic medicine and surgery in the University College, London, &c. London: James Walton, 1869. Small 8vo, pp. 309.

3. The practice of medicine. By Thomas Hawkes Tanner, M.D., F.L.S., member of the Royal College of Physicians, fellow of the Royal Society of Literature. In 2 vols. The sixth edition, enlarged and thoroughly revised. Henry Renshaw, 1869. 8vo, pp. 656 and 645.

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6. As regards protoplasm, in relation to Professor Huxley's essay on the physical basis of life. By James Hutchinson Stirling, F.R.C.S., LL.D., Edin. Edinburgh: William Blackwood & Sons. Sewed, pp. 68.

7. Journal of the Scottish Meteorological Society. Edinburgh: William Blackwood & Sons.

8. A brief paper on the pathology of insanity. By R. C. Shettle, M.D., physician to the Royal Berks Hospital. Reading: Barchant & Beecroft. Sewed.

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17. Scarlet fever; a manual for mothers and nurses. By John Marshall, M.R.C.S., licentiate Royal College of Physicians. London: John Churchill. 12mo, pp. 36.

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30. Lectures on surgery. By James Spence, F.R.S.E., surgeon to the Queen in Scotland, professor of surgery in the University of Edinburgh, &c. Part II. Edinburgh: Adam & Charles Black, 1869. 8vo, pp. 663.

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36. A practical treatise on the diseases of children. By Alfred Vogel, M.D., professor of clinical medicine in the University of Dorpat, Russia. Translated and edited by G. H. Raphael, M.D., attending physician to the Eastern Dispensary for the Diseases of Children, New York. D. Appleton & Co., New York, and 16, Little Britain, London. 8vo, pp. 603.

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DR. WILLIAM MAC CORMAC,
Amputation through the Knee Joint.

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PART I.

ORIGINAL COMMUNICATIONS.

ART. XI.—*On Amputation through the Knee-Joint.* By WILLIAM MAC CORMAC, M.A., M.R.I.A. ; Fellow of the Royal College of Surgeons of Ireland; Member of the Senate of the Queen's University; and Surgeon to the General Hospital, Belfast.

AMPUTATION through the knee-joint or, more correctly speaking, disarticulation is an operation of very ancient date. The very oldest authors allude to it. Indeed, it would naturally suggest itself as a ready mode whereby to remove a diseased or injured limb. That remarkable surgeon, Fabricius Hildanus, to whom are attributed many great surgical achievements, is alleged to have performed this amputation in 1632, and he speaks of it as an acknowledged procedure amongst contemporary surgeons. Hoin of Dijon, about 1764, performed the operation, by means of a long posterior flap, similar to that more recently recommended by Mr. Syme. J. L. Petit, amputated after a similar fashion on two occasions, once, it is said, disarticulating because there was no saw at hand wherewith to divide the bone.

Blandin, who followed Hoin's plan, recommended a counter opening at the base of the long posterior muscular flap for the escape of pus. He mentions as a serious objection that the portion of the soleus muscle, which helps to form the flap, always mortifies.

Brasdor especially extols the operation in an elaborate memoir, published in the Transactions of the French Academy of Surgery, where, amongst other matters, he points out how the circular, or transverse operation as Brasdor named it, may be modified by taking more skin from the front of the limb, according to the seat and extent of the injury. He thus indicated, in short, the modern form of operation.

To Velpeau, however, is conceded the claim of having introduced amputation through the joint into modern surgery. In 1829, he published a memoir enthusiastically commending it, citing fourteen cases, out of which there were thirteen cures. His subsequent experience led him to modify the sanguine expectations he had at first framed respecting disarticulation at the knee, but he maintained to the last the excellence of the operation. Malgaigne expresses the opinion, in his *Manual of Operative Surgery*, "that the operation has been too lightly abandoned, that it possesses every advantage over amputation in the continuity of the shaft of the femur, especially that of leaving the patient freedom in, and command over, the motions of the hip-joint." More recently still American surgeons have devoted much attention to this subject, notably Drs. Smith, Markoe, and Brinton. In our own country, Mr. T. Holmes and Mr. Pollock of St. George's Hospital, and several others, have recognized the merits of this form of surgical procedure. Mr. Samuel Lane was the first, I believe, to perform the operation in England, which he accomplished most successfully in 1857. Before this it had been done in the Glasgow Infirmary in 1847. I do not know to whom in Ireland is to be attributed the first disarticulation of the knee-joint. Amputation close to the joint is not infrequent.

Amongst the advantages which Dr. Markoe, in one of his valuable contributions, claims for disarticulation at the knee are:—

1. The useful character of the resulting stump.
2. The small shock attending the operation.
3. The lessened section of the tissues.
4. The non-exposure of the muscular interspaces and diminished risk of suppuration in the thigh.
5. The comparatively few vessels requiring ligature.
6. The attachments of the thigh muscles being mainly preserved.
7. The absence of muscular retraction.
8. The diminished risk of osteomyelitis and exfoliation, from the femur not being sawn across.

And here it may be mentioned that under the term "amputation

at the knee," two different operations have often been confounded, operations different in character, and likely to differ, as will afterwards be shown, in their results. These are, true disarticulation of the joint, without interference with the articular surfaces of the femur or patella, and amputation through the condyles of the femur, removing more or less of that bone, and, generally speaking, removing the patella also.

Of the latter form are Syme's amputation at the knee, and Mr. Carden's modification of it. Deploring the great fatality of thigh amputations, estimated by him at from 50 to 70 per cent., Mr. Syme re-introduced Hoin's operation, recommending the femur to be sawn through the condyles in order to avoid the many risks which he attributed to dividing the medullary canal. The long posterior flap made of the whole of the calf was nevertheless attended by so many and so serious inconveniences that Mr. Syme felt constrained to abandon this mode of operating. Mr. Carden, of Worcester, some time after, advocated a similar operation so far as the bone is concerned, but he made a long anterior integumental flap, sufficient to cover with ease the entire of the cut condyloid surface. By this plan, which Mr. Carden, at the time of publication, had followed for many years, he achieved marked success, and Mr. Syme acknowledged the method to be a great improvement upon his own. In both these operations the patella is removed.

This form of operation, then, is very different from disarticulation at the knee where, when sound, the cartilages of incrustation on the end of the femur, and on the patella are not interfered with.

The method of amputation through the joint employed at different periods, has, as already indicated, varied much. The long posterior flap of Hoin and Blandin would never now be accepted through choice. Its weight and liability to gangrene, added to the difficulty of keeping it in situ, are sufficiently strong objections, and there are others as weighty. Velpeau adopted a circular cut, four fingers breadth below the joint. This is, however, inconvenient, and leaves a cicatrix on the face of the stump. This latter, and strong objection, subsists against the otherwise excellent operation by two lateral flaps. The operation practised by M. Baudens approaches very nearly the plan now most generally accepted. This eminent French surgeon recommends an oval sweep to be made round the limb extending in front five fingers' breadth beneath the inferior border of the patella, and posteriorly three fingers' breadth below this level. Twice successfully did M. Baudens operate in this

way. In one instance the cartilages were not interfered with, and the stump healed over without any exfoliation taking place; in the other the condyles had been injured by a musket ball, and required removal. Dr. Nathan Smith successfully performed this amputation for the first time in the United States in 1824, and therefore before Velpeau. He removed the limb by forming two flaps anterior and posterior, exactly equal in size. The base of the flaps was half an inch below the line of the articulation, and their extremity corresponded to the insertion of the ligamentum patellæ. Dr. Stephen Smith in the last January number of the *American Journal of Medical Science* recommends a very ingenious form of incision, which for this and other operations is well worthy of consideration.

Such, then, are some of the ways in which disarticulation at the knee has usually been practised heretofore. In quite recent times, in the opinion both of American and English surgeons, one plan of operation is preferable, when at all practicable, to all the rest, and that is a long anterior tegumentary flap, and a shorter posterior flap, while, when healthy, the cartilages of incrustation on the femur, and the patella should remain absolutely untouched. This was the method I myself adopted when I had occasion to perform this operation.

The points of importance in connexion with it are that the anterior flap should, at its base, be nearly or quite two-thirds of the circumference of the knee, just below the patella, and that its length should correspond to at least half its breadth. Further, that the incision to form this flap should commence and terminate on the inner and outer sides of the limb, one inch below the line of the articulation in the adult. To express this in another form, the anterior flap should commence opposite the posterior margin of one condyle of the femur, and extend across the limb about four or five inches beneath the patella to a similar position as regards the other condyle. The superior extremities of this incision should be an inch below the semilunar cartilages. This flap having been dissected up so as to include all the soft tissues in front of the knee, the flexed joint should now be opened as near the femur as possible. By this means all superfluous synovial membrane is cut off, and the semilunar cartilages are not separated from the tibia, as they might otherwise readily be, thus necessitating a separate dissection for their removal. The crucial and lateral ligaments are now with facility divided, the knee being still maintained flexed. As soon as these are freely cut, the knife should be laid flat behind the tibia,

the limb extended, and a thin posterior flap, about half the length of the anterior, rapidly formed by transfixion. Care must be taken in so doing to avoid locking the edge of the knife against the projecting head of the fibula. The posterior flap may be made integumental merely by dissecting it up from without, as in making anterior flap, and then dividing the rest of the soft parts by a circular sweep of the knife. Very few arteries require ligature. I have, however, always found it expedient to tie the popliteal vein. It is of the utmost importance to avoid heavy or tight dressings. These should be always very light, and any tension promptly relieved, should it take place.

Having said so much by way of introduction, which I trust may not prove too tedious, I now purpose briefly relating the particulars of four cases, in which I performed disarticulation of the knee, and of one case in which I performed Carden's amputation.

Case I.—The first case in which I amputated through the knee was that of a man aged thirty-one, admitted under my care to the General Hospital, April 8th, 1865. For three years previously he had been suffering from pain in the left knee, more severe at night, and on putting the foot to the ground. No other symptom was complained of for two years, when some swelling appeared and the pain grew worse. Only three weeks before admission was he compelled to give up work. When I first saw him there were considerable swelling in the knee and severe pain. Appropriate treatment was ordered, under which he improved very much for a time. On May 31st very serious symptoms suddenly supervened. The patient had a rigor, was very feverish, the leg became œdematous, and the joint swelled. Acute suppuration in the joint, it was soon seen, had taken place. On June the 5th a free opening was made, and a large quantity of matter flowed from the joint. On introducing a probe it passed into softened cancellated tissue of the head of the tibia, to which bone the symptoms were all along chiefly referable. Although a very hopeless operation in similar cases, the patient's general condition was such as led me to propose immediate amputation. Unfortunately he would not hear of it until June 10th, when his state was very bad, but he then urged me to relieve him were his chances never so bad, and after a consultation I did so, although some symptoms, not unlike those ushering in pyemia, had been manifested. The patient being chloroformed I made an oval anterior flap six inches long, opened the joint and made a short

posterior flap by transfixion. There was pus in the joint. On examining the end of the femur it was found that the cartilage had been removed and replaced by granulations. No diseased or softened bone was felt, and it was decided to be better not to inflict further injury on the femur by slicing its end off. The patella was similarly affected, and it, too, was left untouched. The disease had commenced in the head of the tibia, to which it was mainly confined, the femur being only secondarily affected. An abscess had formed in the head of the bone which afterwards found its way into the joint, and the action in the femur was an effort to protect itself from injury. The popliteal artery and one other vessel were secured by acupressure needles (inserted after Simpson's third method). The wound was then drawn together by sutures, and the patient removed to bed, water dressing having been applied. As might be expected there was considerable tendency to general oozing, very difficult to check, and returning two or three times. The application of ice was had recourse to. An examination of the part removed justified the diagnosis. A pyogenic cavity was found in the cancellated structure of the head of the tibia. Around this the bone was softened and inflamed, and a round opening communicated through the centre of the external articulating facet of the tibia with the joint. It was, no doubt, in consequence of this perforation suddenly taking place that the violent inflammation all at once occurred in the joint. The day after the operation the symptoms of pyemia in its most acute form set in. Violent rigors, hot fits, and sweating of the most profuse kind, induced me to anticipate a speedily fatal result. No treatment is of much avail in acute pyemia. I simply administered large doses of opium. I need not weary the reader by a detailed history of the case, further than to add that on dressing the stump on the third day, the anterior flap was observed to be fringed by a gangrenous border. No healthy action ever took place in the wound. No clot formed in the popliteal artery, which pulsated violently in the open sloughing wound. Fortunately the needle kept its place even under these unfavourable circumstances, showing that it does not cut the arterial coats so speedily as some has alleged. Harbinson died on June 16th, exhausted by the disease. The operation, perhaps, hastened the fatal issue, but that even is doubtful, as for three or four days he asserted he had been greatly relieved by it. That his life would have been saved had he assented to an operation when it was first proposed to him is perhaps unlikely, but, beyond question, a better

prospect of recovery would have been afforded. No general *post mortem* examination could be obtained, but I seized an opportunity of examining the stump. Unluckily the dressings had been displaced during the removal of the body to the dead-house, and the needle on the popliteal artery torn off with them. The muscles were dark in colour, and the popliteal vein was filled with thin greyish pus. There were two small secondary abscesses in the thigh.

Case II.—On the 24th May, 1866, the anniversary of the Queen's birth-day, I was summoned some miles to the country, by Dr. Fulton, of Saintfield, to see a poor man called Quin. He was a commercial traveller of very intemperate habits, and about fifty years of age. Whilst celebrating the anniversary by firing off a small cannon for the amusement of his children its contents accidentally lodged in his left leg. The cannon was eighteen inches long, and of two inches bore. The charge consisted of six or seven ounces of powder, in addition to which he had foolishly introduced a quantity of small stones, picked off the walk in front of his door. It is supposed that the cannon swerved just before being discharged. At all events, he received the entire contents in his leg.

The result was a horrible lacerated wound, six or seven inches long, and the entire breadth of the limb. Above, it reached nearly to the tuberosity of the tibia. Both bones were extensively comminuted, and a piece of the tibia two inches long was lying loose in the centre of the wound. I thought it likely the fracture extended into the joint, but could not make myself certain of its doing so. The soft parts were completely disorganized, and blackened by the proximity of the poor man to the discharge.

The question of saving the limb was not entertained, the only point to determine was where to amputate. Considering the nature of the injury both to the bone and soft parts, I at once decided against amputation in the leg. The bone was probably split up into the joint, and the soft parts were so much bruised and lacerated that I thought they were not safe to furnish sound covering, and this could only be obtained from the calf. I decided on amputation near the joint by Carden's plan. Had the covering to be obtained been sufficient I would have preferred disarticulation. An operating table was extemporized, some candles provided, and the patient being chloroformed, I made an anterior flap as long as possible, down in fact to very near the margin of the wound. This

flap was dissected up, the joint opened, the crucial and lateral ligaments were divided, as also the tissues posteriorly by cutting almost directly outwards, I then sawed a slice off the condyles. The popliteal and the azygos articular arteries alone required ligature, unless we can reckon a small sural branch. I also passed a ligature round the popliteal vein, as the bleeding from it was not arrested by pressure, and I have no reason to believe that the ligature of a vein in a stump is hurtful, however large. Interrupted sutures and water dressing having been applied, the patient was removed to bed. I dressed the stump on the third day. Union had taken place between the flaps on the right side, but elsewhere a very unhealthy discharge was being poured out. The thigh was mottled all over with yellow and purple patches. The conjunctivæ were yellow. The pulse was 120, and although he had slept well during the night, he continued in a somnolent state during the day. It was at this time that the sulphurous acid treatment, recommended by Professor Polli, was being tried in septic diseases. Fearing the advent of pyemia, although the patient was located in a country cottage, and not in a crowded hospital, I ordered the bisulphite of soda in thirty grain doses frequently repeated, as a prophylactic, in which capacity, this, as well as some other remedies, has been alleged to act admirably. I also ordered ample nourishment, and a moderate quantity of wine.

Next day, the 28th, there was to be seen at the edge of the anterior flap a portion, which appeared to be losing, if it had not already lost its vitality. On the 29th the pulse was 120, the tongue was clean and moist, there was a plentiful discharge of thin unhealthy pus. On examining the stump, the anterior flap was found to be fringed with a gangrenous margin half an inch deep and three inches long. This did not extend any further, but the symptoms of pyemia increased, the jaundice persisted, the breath acquired a sickening sweet odour, he had severe sweating, the belly became greatly swollen and tympanitic. The stomach rejected everything, and uncontrollable diarrhea set in. He gradually, in short, became weaker, and died on the 6th of June, a fortnight after his mishap. The double shock of the accident and operation, and above all, the man's antecedents, contributed to cause the unfortunate termination. Probably even a smaller amount of injury, or a less serious operation would have been followed by a like result.

Case III. presents many features of interest, apart from the

method of operation adopted. It is that of the young man whose photograph accompanies this paper.

William J. Allen, a tall lad, seventeen years of age, rather delicate looking, and with a very anxious cast of countenance, was admitted to hospital, under my care, in last October, with a large tumour occupying nearly two-thirds of the right tibia.

He states that some eight years ago, when only nine years old, a small swelling which has uniformly, though slowly, increased ever since, formed over the tibia. Within the last twelve months the rate of growth has been more rapid. There was never much pain, and generally no pain whatever, attending it. The tumor occupies the inner and anterior aspect of the tibia. Above, the tumor terminates four inches below the head of the bone, below it extends within five inches of the ankle. The tibia above the tumor is not quite free from enlargement, below it very much increased in size. The surface of the tumor is smooth, of bony hardness, and channelled by the superficial veins. It rises gradually up from the bone, which seems clearly to be expanded over it. On the lower and front part is an indolent superficial ulcer about three inches square. This began some fifteen months before, and has since been slowly enlarging. It is not painful, and the amount of discharge from it is very trifling. It seems to have been originally produced by the friction of the clothes against the part.

The diagnosis, though not quite simple, indicated a bony or cartilaginous benign tumor growing in the bone, which was expanded over it.

After consultation it was decided to amputate. The boy could, no doubt, walk about pretty well, there was no swelling in the foot, he complained of no pain, nevertheless his general health was beginning to suffer a little, the tumor was increasing more rapidly, and the lad himself being most anxious to have the leg cut off, I could see no advantage in delay.

The operation was performed on November 6, under chloroform. A long anterior flap, nearly down to the tumor, was first made. The incision was commenced on the inner side, three quarters of an inch below the line of the articulation, and well back near the hamstring tendons. The cut terminated at a corresponding point on the outer side of the joint. This flap was rapidly raised up, the joint opened, bisected, and a thin short posterior flap formed, after the limb had been extended, by cutting from within outwards. With

the exception that it is placed just a little too low down, the line traced on the cast in the photograph represents the direction of my incisions. This cast was taken before the limb was removed, and gives a good idea of the form and size of the tumor. I thought it would lend further interest to the picture if it were photographed at the same time as the patient upon whom the operation for its removal had been executed. At first sight it would appear that these flaps are made needlessly long. Such is not the fact, the amount of retraction is considerable, and if they were made shorter great straining must result, as the flaps often retract very much. If you find the covering insufficient you will then have to perform Carden's operation by cutting off the condyloid end of the femur. This is what I felt compelled to do in Quin's case. In the present instance by the time the popliteal and four other vessels had been tied, the patella was drawn up some two inches, and when the flaps were brought together they met at the posterior part of the stump without traction certainly, but also without the smallest redundancy. Neither on the femur nor on the patella was the cartilage interfered with. Carbolized dressings were applied to the stump after the flaps had been washed and sutured. Two days after the operation, the patient having been going on well in the interim, his pulse rose to 120, he became feverish, and complained of great pain in the stump. Fearing there had been some oozing of blood, distending the flaps, I partially slit up the dressings, through a space of some inches, at each angle, found the edges of the wound firmly adhering together, and no appearance of distension. This allayed my fears, and I did not disturb the dressing further. The pain reappeared on the fifth day, and I then removed all the dressings. The central portion corresponding to tip of the long flap had become gangrenous to the depth of three-fourths of an inch, by two inches broad. I came to the conclusion at the time that this was perhaps induced to some extent by the pressure of the bandages. A flap so long and delicate requires the very gentlest treatment. I believed, however, I had taken all needful precautions. No extension of the gangrene occurred. The slough was soon thrown off, and the condyles became exposed.

On December 7, a month after operation, I find the following record, which I transcribe from my note book:—"Since last report this patient has been getting on in all ways well. A superficial healthy granulating surface occupies the site of the former slough. All the rest of the wound healed by primary adhesion, and has

since remained sound. It was interesting to watch the gradual process of removal of the exposed cartilage. It seemed precisely analogous to the process, only somewhat slower, which occurs when some part of the cranium is denuded of periosteum—a layer of the skull perishes, and is disintegrated slowly, or scales of bones are thrown off. Granulations form from the sound bone beneath, press upon and absorb the dead portion, or it may be wholly surrounded by granulations, loosened, and cast off in a piece.

"I carefully watched similar changes going on in the exposed cartilage. It seemed gradually to get thinner, and around the edges, granulations springing from the surrounding parts encroached. After a time granulating points appeared through the cartilage itself, rising from the bone beneath. These points extended, and the granulating surfaces gradually fused together into one tract, and all trace of cartilage disappeared. The process is one of slow absorption and substitution. In other cases the cartilage may be thrown off in large exfoliations." "At the present date," the report continues, "any amount of pressure can be sustained by the condyloid surface. The patella remains quite movable, and is drawn up at least two inches above its normal situation. The lad goes daily about the hospital, and out of doors, and is, in fact, in all respects convalescent." He left the hospital soon after. The photograph was taken in the middle of March, about four months after operation.

With a detailed account of the nature of the tumor I need not trouble my readers. The tibia was expanded over it. While above, and especially below, the bone was much hypertrophied. It was in addition two inches longer than the tibia of the other leg. The fibula, though not involved in the growth, was hypertrophied. On making a section of the bone it was found to contain a number of small and large membrane-lined cavities, varying in size from a pea to a walnut, containing bloody serum, and divided from each other by thin bony walls. In the interstices were masses of yellowish white substance, firm and elastic. In some places these were softer and opalescent. A microscopic examination showed this material to contain multitudes of cells of all sizes, spherical, oblong, caudate, fusiform, and triangular, about one twelve hundredth of an inch in diameter.

In short the microscope gave evidence of a malignant type of growth, and to the naked eye the tumor presented the appearance of medullary cancer. Yet the history of the patient's illness, the

almost uniform and slow growth of the tumor during eight years, its external characteristics, and the absence of pain, would warrant a very different conclusion.

Case IV.—This and the next case I mean very briefly to allude to as they are still under treatment. The present case is that of a lad who was admitted to hospital on January 24, 1870. Ten days before admission he had a rigor followed soon after by pain, redness, and swelling in the left leg. He is a very delicate looking boy, but up to the day he took ill had been constantly at his work.

The disease was at first treated for erysipelas, from which acute periostitis and necrosis needs to be very carefully discriminated, as the only hope of saving the bone in the latter is by very early incisions.

When examined shortly after admission, the limb, from one extremity to the other, was occupied by a bag of fluctuating pus. There was great swelling about the ankle, and a superficial ulcer over each malleolus. A free incision was at once made, and matter, mixed with blood clots, evacuated to the extent of half-a-pint. On introducing a finger into the wound, it was found that the pus had been contained between the periosteum and the bone from which the former was detached, as far as the finger could reach, in the axis of the bone as well as all round it. The boy was much exhausted, and I felt certain the entire diaphysis had perished. He did not come under my care till he had been a week in hospital, and I then commenced antiseptic dressings. The disease, however, had largely implicated the ankle-joint, and it was showing symptoms of travelling upward to the knee. The discharge continued very profuse, and the pulse rose as high as 140. His condition had become desperate. As a last resort, and as affording the only prospect of saving his life, I determined, with the consent of my colleagues, to amputate on the 8th February. At this time he was so weak I feared he might not survive the shock of the operation; but it was evident, from the rapidity of his downward course, he could not survive many days if he were not relieved in some way. What I feared chiefly after operation was pyemia. I believe it is especially likely to occur in such cases as this one. For this reason, and believing the shock to be less, I determined to amputate through the joint, although the parts of which I had to form my anterior flap were œdematous. This was not only so, but

in forming the flap I cut through a small abscess. The operation was performed in the usual way under chloroform. Seven vessels were secured by carbolized catgut ligatures, including the popliteal vein, and the ends cut short. He rallied well after the operation, the pulse came down, and he expressed himself as being greatly relieved. The subsequent history is, that the anterior flap sloughed in that portion where the abscess and œdema were situated, the condyles were exposed, and the cartilage became slowly replaced by granulations. An abscess formed high up in the thigh, and his convalescence has been very tedious. I trust, however, now, that he is in a fair way to a good recovery. He has been up daily and out of doors. The amount of disease, considering that it had been of only three weeks' duration, which the amputated limb displayed was remarkable. The entire shaft and lower epiphysis were necrosed, and the superior epiphysis was beginning to separate. The periosteum, which in one place had formed some new bone, was completely detached throughout. It presented a thick velvety injected surface. The ankle-joint was completely disorganized, and the cartilages of incrustation were thrown off. A similar condition existed in all the other tarsal joints, and the bones of the tarsus were softened and inflamed. The tarso-metatarsal joints were not affected, and the disease had not extended so as to implicate the knee-joint.

Case V.—This man, although only twenty-seven, looked very much older. He had contracted syphilis nine years ago, and has suffered severely from its manifestations ever since. He was treated at different times by mercurials in many forms, and he has taken iodide of potassium in doses varying from five grains to sixty grains. He submitted to a protracted course of syphilization. He is marked all over with the scars of the syphilitic ulcerations, which have formed on different parts of the body. For the last two years the disease appears to have become localized in the left leg and foot, which is treble the normal size, covered with tubercular masses and large painful ulcers. The agony he has to endure caused him to have recourse to opium, and he admits taking four ounces of laudanum daily. His friends say he takes a good deal more. One of the toes has just dropped off, and the others seem on the point of doing the same thing. The poor man placed himself in hospital, under my care, and begged that the limb might be removed at any risk. The tibia had been the seat of antecedent disease, and there were small ulcers in different parts of the leg. The patient himself

was very urgent that the whole leg should be removed. I therefore determined to disarticulate, and performed the operation under chloroform, on February 12, 1870. I made the flaps as in the other cases, commencing nearly two inches below the line of the articulation. Both the anterior and posterior flaps were made rather longer than before. Notwithstanding this, the tendency to retract was very great, and the soft parts would have been pulled right away from the condyles had they not been restrained. It is mentioned by several writers as one of the great recommendations of this form of operation, that retraction of the flaps does not occur. In the present instance this objectionable tendency was present in a most aggravated form, and I have noticed it, though not to the same extent, in previous instances.

The main artery and vein, and other vessels were secured with carbolized catgut ligatures, the ends of which were cut short, and carbolized oil dressings were applied. I previously inserted thick catgut stitches in the flaps, which I find do not hold a sufficiently long time.

There was no shock, and no febrile reaction took place. The patient was doing in every way well, except that the edge of the anterior flap became gangrenous—just the edge—but more than sufficient to prevent union. The catgut sutures after a time gave way, and in the depths of a very unhealthy wound the popliteal artery was seen beating with all its force. This was a week after operation. Presently the situation of the vessel was covered by healthy granulations, and in another week the pulsations became weaker; but it was not for more than three weeks that they subsided so far as to dissipate all fear of secondary hemorrhage. Meanwhile, no trace of the catgut ligatures was seen. They seemed to have been covered over with the granulations, and to have become absorbed.

At the time I write the man is quite convalescent. There is still a considerable extent of granulating surface to heal, but this is closing, and when the stump is soundly healed, I have no doubt it will prove an excellent and useful one.

We may now proceed to consider the value of this operation of disarticulation at the knee-joint. We find very various opinions entertained about it. I believe that, like other things human, it is far from being perfect, but the weight of evidence is strongly, I think, in its favour. With very few exceptions, we may conclude that the mode of operation here described is that which will most generally be adopted. There is doubtless a latent dislike to bisect

a large articulation like the knee, conversant as we are with the very serious consequences which attend the most trivial injuries of it. As a matter of experience, however, the free opening of the joint in amputation is apparently followed by no special consequences such as, *a priori*, we might anticipate. Danger from this cause must be of very rare occurrence. The two forms of injury, in fact, belong to different categories.

Another objection, felt strongly by some, rests in the presence of the articular cartilages, which must needs, they say, die and exfoliate, causing delayed healing, tender painful stump, and endless trouble and danger. Mr. Butcher, for instance, says in his *Operative and Conservative Surgery*, "I have no doubt whatever that in all cases the cartilaginous surface of the femur should be removed." He recommends a curved section of the bone as giving as good a surface for support as where the cartilages remain untouched. Now after disarticulation one of two things will happen, either immediate union takes place, or some portion of the anterior flap may slough, and the cartilage of incrustation become more or less exposed. This latter is certainly the more frequent result. Indeed, Dr. Brinton considers sloughing of the margin of the flaps occurs to some extent in most cases. But supposing union do occur by the first intention, the cartilages do not exfoliate, and do not give any further trouble. In the case recorded by Mr. Lane, and in other cases, there was free motion both of the patella, and of the flap, over the subjacent surfaces. Immediate union took place without exfoliation of cartilage. In all three of my cases that survived, the patella remained moveable, and in Allen's case the flap was partially moveable also. In process of time the cartilage no doubt becomes altered and absorbed, as tissues always do for which there is no longer any use, and eventually the flap and the bone will become joined by some connecting medium, but meanwhile the articular cartilage affords a smooth desirable resting place for the flap. But suppose a less fortunate issue to the operation, suppose union do not take place, that there is retraction of the flaps in consequence, and exposure of the cartilage. In that case exfoliation will occur, but not for the lapse of two or three weeks, not in short until the slough have been cast off, and the stump may be supposed to be granulating healthily. In the interval I consider the cartilage has served a most valuable purpose. It is an unabsorbent protective to the end of the bone, shields it from the influence of the hurtful secretions of the wound, and by the time it is exfoliated

or disintegrated, the end of the bone is covered and protected by a granulating surface. This, in my mind, is a condition which contrasts most favourably with an operation, where other conditions being alike, the bone, in place of being thoroughly protected, is divided through its cancellated structure, whose patulous porous nature is just such as to invite the entrance of the noxious discharges of the wound. I am satisfied that this condition, other things being equal, is calculated to induce pyemic poisoning.

No advantage is derived from sawing off the condyles. When healthy they should most certainly be retained. The presence of the articular cartilage does not interfere with the healing of the wound, and if primary union do not take place the safety of the patient is better secured by the presence in the wound of a cartilage-covered bone, than by an exposed surface of cancellated tissue. Dr. Stephen Smith, the latest writer on the subject, adverting to this point, remarks that "the methods of Syme and Carden, involving excision of the condyles, are extremely objectionable, as well as entirely unnecessary. The great source of disappointment which I have met with is the want of sufficient vitality in the long flap. In every instance in which I performed the operation the edge of the flap sloughed more or less. In all the cases, save that of Allen, there were doubtless constitutional causes present sufficient to account for the gangrene, still, as shown in that instance, there will be, under even favourable circumstances, an almost fatal liability to sloughing. This is due, of course, to the length, tenuity, and the inadequate arterial supply of the anterior flap.

Possibly greater care in guarding against the hemorrhage of reaction, in dressing the stump, or the abstinence from dressings of all kinds, care, in short, to prevent any pressure upon the flap either from within or from without might serve to avert this unfortunate contingency. As helping towards the same end, Mr. Pollock has pointed out how important it is not to cut a single line higher up between the anterior and posterior flaps than what is absolutely necessary, and that the incision should not commence above the lower edge of the condyles of the femur. I was myself so much impressed with the necessity of this precaution that I invariably began my incision on a plane very much lower than that indicated by Mr. Pollock, namely, nearly one inch below the margin of the head of the tibia. It must also be recollected that the broader the base of the flap is made the less the risk of gangrene will prove. In judging of the results of the operations I have performed, it must

be conceded that, with the exception of Allen, it would be difficult to find more unfavourable cases.

Harbinson was suffering from long continued disease of the tibia, in which an abscess, subsequently bursting into the joint, had formed. He refused operative interference for a long time, and claimed it when too late. Pyemia, of a very acute type, though it had not declared itself at the moment of amputation, shortly after set in, and he was soon carried off. The operation did not appear in any way to shorten his career.

Acheson, a sickly strumous lad, was attacked with acute necrosis of the entire shaft of the tibia, and destructive inflammation of all the tarsal bones. An amount of damage, such as is rarely met with, took place in the short term of three weeks. When the operation was proposed he appeared to be almost in a dying state. The nurse, a most experienced woman, said "that boy, I fear, will not come back to the ward alive." Yet, though the prospects seemed so hopeless, there was not only no shock, but almost immediate relief was experienced, and I expect he will yet do well. Crawford was but little better, worn out, as he was, by nine years' constant suffering, a confirmed laudanum drinker, the tissues of the limb diseased, the muscles only to be compared to living adipocere. What was to be expected in such a case after a capital operation? Yet here, too, there was complete absence of shock. In spite of the partial gangrene, in spite of the ominous beating of the popliteal artery, he was able to sit up daily a fortnight after the operation. He is now progressing to complete recovery, and will in time possess an admirable stump. He has since left the hospital.

A sufficient interval has now elapsed to show what Allen's stump is like. It was photographed some four months after the operation, and no graphic power is needed to describe its great length and consequent power, its broad rounded extremity, covered with thick integuments, and well calculated to sustain pressure, while the line of cicatrix is quite posterior and out of the way.

Mr. Pollock, with good reason, speaks of the stump resulting after amputation at the joint, "as most satisfactory, and the adaptation of an artificial leg as most easy." Dr. Brinton goes further when he states that "patients, surgeons, and surgical mechanicians are agreed, their testimony is nearly unanimous as to the serviceability of the stump, and its fitness for the adaptation of an artificial limb."

One remarkable instance in support of these views, I may perhaps be permitted to quote:—"E. K. P., aged twenty-one,

received a shell wound of the leg at the battle of Cedar Mountains, U.S., Aug. 9th, 1862. Primary amputation at the knee was performed by Surgeon Bentley, U.S. Volunteers. The articular surface of the condyles was sawn off, the patella was left. The patient recovered without accident in four months. He has since been engaged in four battles, and on one occasion was captured, and obliged to walk fifty-four miles between sunrise and nine o'clock p.m. The stump is painless and well formed, and he can walk, or dance, mount his horse and ride, with perfect facility."

The rate of mortality attendant on amputation through the knee, as compared with amputation of the thigh, is, I think, somewhat difficult to arrive at. I question if there be a sufficient amount of material wherewith to form a correct estimate. The largest number of cases is contained in the invaluable reports issued by the United States' Surgeon-General. In Circular No. VI. Surgeon Otis records 132 cases of knee-joint amputations up to 1864, with a death-rate of 48 per cent.

In Dr. Brinton's most interesting communication, already more than once referred to, that surgeon relates that further returns have been made to the Surgeon-General's office, which raise the number of knee-joint amputations performed to 211. Of these 106, or 50·2 per cent., proved fatal. Many of these additional examples, Surgeon Otis explains, are cases of primary amputation, collected from special reports, which died before reaching hospital.

Mr. Pollock has collected forty-eight cases from various sources, operated upon in England, with thirty-six recoveries and twelve deaths, or thirty-three and a third per cent.

Dr. Markoe gives the following table, showing the results in the aggregate of American and foreign cases of amputation at the knee-joint:—

	Cases	Recoveries	Deaths	Mortality per centum
Primary amputations after accident, -	59	34	25	42·37
Secondary amputations after accident, -	37	23	14	37·83
Secondary amputations for disease, -	62	48	14	22·58
Cause and period of disease undetermined,	6	6	—	—
Total, - -	164	111	53	35·31

Mr. Pollock has had himself nine cases and one death. Of my four cases one only proved fatal, besides the Carden's amputation.

So far, too, as the comparison has gone, it would appear from the cases collected by Dr. Brinton that the ratio of deaths is somewhat greater when the condyles have been removed than when they are left intact, and on reading over these cases one cannot fail to be struck with the relative frequency of death from pyemia amongst the former. Against this, however, may fairly be quoted Mr. Carden's remarkable success of thirty-one cases and five deaths.

Undoubtedly, the death-rate in this operation will contrast favourably with amputation of the thigh, forming the estimate from cases similarly circumstanced.

In the late American war, Surgeon Otis reports 1,597 completed thigh amputations, and 1,029 deaths, or 64 per cent., and in civil practice the mortality attendant on primary amputation of the thigh cannot be much less than 60 per cent. Fifty per cent., or, according to others less, is the ratio for amputation through the knee. If Dr. Markoe's rate of mortality of 22 per cent. for amputations through the knee-joint for disease be correct, this also will bear favourable comparison with pathological amputations of the thigh in civil practice.

Dr. Hamilton, in his *Military Surgery*, considers that disarticulation at the knee has proved more successful and more safe than amputation either immediately above or immediately below the joint. And Dr. Stephen Smith thinks it established that this operation is less fatal than amputation in the upper third of the leg.

I do not agree with these authorities. I would not feel myself justified in amputating at the joint, if it were practicable to do so at any point lower down. I think disarticulation should never from choice be substituted for amputation of the leg. The cases, then, to which it is applicable must prove comparatively rare. They are injuries to the knee-joint, not involving the femur, or only involving it to a slight degree, extensive disease, or severe compound fracture, involving the whole of the tibia, but without great destruction of the soft parts over the front of the limb.

In very many cases where amputation of the leg is inadmissible, the state of the parts will be such as to necessitate amputation of the thigh. Possibly this may be one of the chief reasons why amputation through the knee-joint has for lengthened periods been completely abandoned, to be from time to time revived as surgeons happen to meet with cases which appear eminently suitable for it.

Since writing the foregoing paper I have again performed

disarticulation at the knee-joint with every prospect of a most satisfactory issue. It was in the case of a little girl of thirteen years old. Six months ago, acute periostitis and necrosis of the tibia took place, and a sequestrum, comprising half the shaft of the tibia, was afterwards removed. An interval of five months passed, and no new bone was deposited. The limb became greatly deformed, and was quite useless. Finding that there was no hope of preserving a member of any use to the patient, and finding that the knee-joint was becoming involved, that there was free lateral motion in it, and some grating between the head of the tibia and the femur, I considered amputation not only expedient but necessary. Her general health, too, was beginning to give way, and the wound made to remove the necrosed tibia had never healed. The little patient being chloroformed, I first made a posterior cutaneous flap, about three inches long, and dissected it up to the level of the articulation, including in it all the subcutaneous tissues down to the muscles. I then made an anterior flap somewhat longer, opened the joint, and cut the rest of the parts by a circular sweep of the knife. The flaps were carefully formed with a scalpel, cutting at right angles to the surface, and their edges were thus quite thick. The vessels and nerves were divided transversely. Catgut ligatures, and Mr. Lister's lac dressings were employed. Although the flaps were nearly of equal length, the anterior one folded completely round the end of the femur which was not interfered with, and met the retracted posterior one an inch and a half above the end of the condyles. The cartilage on the femur was sound, that on the tibia completely disorganised.

The total absence of shock after the operation was very evident when I visited the child in the evening. Her face was perfectly tranquil, she was not suffering pain, and her appearance betrayed not the slightest indication that a serious operation, or indeed any operation at all, had been performed upon her. Ten days have now passed, and the child has continued quite well. The wound is healing admirably, and there has been no sloughing or trouble of any kind.

ART. XII.—*Some Practical Remarks upon the Treatment of Syphilitic Disease.* By JOHN K. BARTON, M.D., (Dub.) F.R.C.S., &c.; Surgeon to the Adelaide Hospital; Lecturer on Surgery in the Ledwich School of Medicine, &c., &c.

THE principles upon which the treatment of syphilitic disease should be conducted, are sufficiently simple, yet, if I may judge

from cases which have come under my own observation, they are not as generally recognized, or as universally acted upon, as their importance demands. In these principles I do not include the use of any particular therapeutic agent, but simply the general rules which a knowledge of the natural course of the disease, and of the effects produced by certain medicines on the symptoms, have enabled us to arrive at; these form a safe groundwork upon which to determine the line of special treatment for adoption in any particular case. In the following pages I purpose drawing the readers' attention to these principles, and illustrating their practical utility by cases, in some of which the same remedy (mercury), will be seen to have produced, when given upon a *wrong* principle, aggravation of the symptoms, with great deterioration of the general health, and subsequently, when given upon *right* principles, marked improvement of the general health, and a disappearance of the symptoms.

Beforing entering upon this subject, I am compelled by some observations[a] made in a recent communication in the pages of this Journal, briefly to refer to the well worn controversy concerning the distinction which exists between the three forms of venereal disease. The conclusion sought to be drawn from some of the cases referred to in this communication is such, as, if adopted, would throw us back into the uncertainty and confusion from which, thanks to the labours of many independent observers at home and abroad, we have recently emerged. The conclusion to which I refer is thus expressed—"On referring to the accompanying tabulations, it will be seen that, as a rule, all sores, whether soft or hard, were followed by constitutional signs."[b]—Upon referring to the tables as directed we find a certain number of cases in which it is stated that characteristic soft sores existed at the time of admission, and in which subsequently syphilitic symptoms became developed.

Admitting the facts to be as stated, and admitting also, which is not, however, at all proved, that in some at least of these cases the syphilitic symptoms which appeared, were not the return of old symptoms, but the first sign of constitutional infection; still the question remains, what proof is there to show that these sores were the initial lesion of the syphilitic symptoms which followed? There is no proof, for there is no mention made of any examination with

[a] Clinical Review of Cases under Treatment at the Westmorcland Lock Hospital. By John Morgan, F.R.C.S.I., &c. Dub. Quart. Jour., Nov., 1869.

[b] Dub. Quart. Jour., Nov., 1869, page 508.

the speculum to ascertain the possible existence of other sores; nor is there even any careful account of the time which elapsed between the development of the sores, and that of the syphilitic symptoms.

In the absence of all such data there are three ways of explaining the facts recorded in the tables, and it will be admitted we are bound, in endeavouring to explain any phenomena, to adopt the most reasonable and likely, not the most unreasonable and unlikely solution of the difficulty. The *first* explanation which suggests itself at once is, that these "soft sores" had nothing whatever to say to the syphilitic symptoms, being another disease, which prostitutes in the constant habit of promiscuous intercourse, were, to say the least of it, not unlikely to contract, in addition to a syphilitic sore, which they either had not noticed, or, having noticed, considered of no consequence; but which trifling sore was really the initial lesion of the subsequent symptoms. If this explanation be objected to in any instance, the facts before us admit of a second explanation equally satisfactory and probable, and involving no difficulty, which is, that in some cases the "soft sores" were true syphilitic chancres, in which the sign of induration was wanting at the time of observation, for it is well known that *induration*, however valuable a sign when present, is not to be looked upon as absolutely necessary to the syphilitic chancre; and indeed for this reason the names of hard and soft as applied to the infecting and non-infecting sores are very objectionable; but in the tables in which these cases are given, the sores are simply described as "soft sores," or a "number of soft sores," the other signs of *surface, secretion*, &c., being altogether omitted. It seems probable that in some of the cases the former and in others the latter explanation was the true one.

The author of these tables, however, adopts a very different explanation of the simple phenomena there stated, which is—that these "numerous soft sores" were examples of what is commonly called the local venereal sore or chancroid, which, indeed, is very likely; but—strange conclusion—that the secondary syphilitic symptoms followed from them. I have shown that there is not the slightest necessity for any such conclusion, as the cases admit of ready explanation without it; but if adopted we must at once pronounce all the careful and laborious researches which have been made upon this subject, since Bassereau, in 1835, began his observations, up to the present time, to be altogether wrong, and this rather than admit that a certain number of women in the nightly habit of prostitution might have two kinds of venereal disease at the same time.

I leave the reader to adopt whichever of these explanations he thinks most reasonable.

Syphilis in any of its stages may present difficulties of *diagnosis*, but more frequently in its later stages is this the case. The plan generally taken in such cases is to make a minute inquiry into the antecedents of the patient, and the diagnosis is formed very much upon the information thus acquired. This appears to me to be a grave mistake, and one which very oftens leads the surgeon into error. To found a diagnosis in any case upon the history of his symptoms, as given by the patient, is always dangerous, but much more so in syphilis than in other diseases, for so chronic is the disease and so various the symptoms it produces, that the patient may readily deceive himself and the surgeon consequently who trusts to what he tells him.

Under any circumstances of difficult diagnosis, I am convinced that our efforts should be directed almost exclusively to a careful, and, if necessary, a repeated examination of all the signs, which the skin, nails, hair, mucus-membrane of the mouth and fauces, &c., will present, rather than rest any weight upon the history given by the patients. For obvious reasons some patients will deny the possibility of their having contracted[a] syphilis, while others will unintentionally mislead in the opposite direction. Several cases have come under my observation in which the patients have given a history of primary sores, followed by an eruption, which, upon closer investigation, turned out to be a rash produced by copaiba, or an ordinary

[a] A brewer's man, aged twenty-seven, married, and accustomed to drink freely of beer, presented himself at the Adelaide Hospital, upon the 25th of September, 1869. He had a sore upon the penis, partly upon the reflected prepuce, partly on the glands, red, smooth, excavated and indurated, glands in groin enlarged and hard, complained of weary pains in his bones, worse at night; post cervical glands enlarged, and hair commencing to fall; no eruption on skin, nor any sore throat. When asked as to when he contracted the sore he said he first perceived it about three or four weeks previously, but positively denied ever having had connexion with any woman but his wife, who was the mother of several healthy children. Totally disregarding these statements and relying upon the distinctive features of the disease, which were well marked, I made the diagnosis. "Syphilis in the stage of premonitary symptoms before the ushering in of secondary lesions," and to confirm this took the patient into the hospital, where he was kept in bed and given a diaphoretic mixture; within a week a well marked papular eruption began to make its appearance, accompanied by sore throat, and simultaneously the fever and pains were relieved. The future progress of the case, up to the present time, amply confirmed the original diagnosis; but to the last, although often questioned, the patient denied the possibility of his having any venereal disease; but when in his hearing, I urged upon the students the necessity of making a diagnosis in such cases, quite irrespective of the history given by the patient, he was observed to relapse into a broad grin.

skin affection (scabies, excema, or purpura). On account of this double source of error, I view with suspicion any diagnosis founded upon the previous history. *The signs which we ourselves have an opportunity of observing, are the only reliable grounds of a diagnosis in any difficult case,*[a] and by carefully noticing them we will always be able to come to a definite conclusion, and either confidently enter upon treatment, or on the other hand dismiss the subject of syphilis from our patient's mind.

I have elsewhere[b] stated my views of the *treatment* of syphilis; these views have been confirmed by more recent experience, while at the same time I am convinced that in order to make a change in the old method of giving mercury rapidly and in large quantities for syphilis, it is necessary to insist upon the principles which should guide us.

The first general rule or principle of treatment which I would suggest, is, that our *object* should be to remove all existing symptoms as completely as possible, *not* to drive out the virus from the system; the virus we only know by its effects, and there is no proof whatever that the virus is directly influenced by any treatment. There is, however, very good reason to believe that the virus is in a large number of cases really eliminated from the system, after the lapse of a considerable interval of time, during which its presence in lessening activity is seen, and there can be little doubt that by removing the symptoms we indirectly aid in this elimination, so that while we confine our treatment to the removal of existing symptoms, we are accomplishing, as far as treatment can, the purification of the system; but this must not be the object we propose to ourselves or our patients. When the symptoms have totally disappeared we have no further safe guide; but might, with the theoretic object of freeing the system from the virus, persevere indefinitely to the destruction of the patient's health, and after all be disappointed by finding a new batch of symptoms appearing when his cure was pronounced complete.

Experience, I think, however, shows that while our object must be simply the removal of the symptoms, yet that in order to do this satisfactorily, that is, to prevent, as far as possible, *a return* of the symptoms, we must persevere with treatment for some weeks after

[a] I find other surgeons have adopted a similar rule. Mr. C. Forster says—"For many years I have enjoined my reporters only to note down what they see."—Guy's Hospital Reports, 1870, p. 162.

[b] Syphilis, &c., Chap. ix., page 200, &c.

all signs of the disease have disappeared; this is not necessary in all cases, but in the majority, especially such as are in the second stage, this is a wise and useful precaution.

The second general principle is, that whatever therapeutic means we employ should be used in a gradual and gentle manner, in contra-distinction to the active and energetic use of the same drugs. The reason of this is, that syphilis is essentially a chronic disease; its symptoms are gradually developed, and continue for a length of time, gradually disappear, and are followed after an interval by another set of symptoms which pursue a similar course. That a remedy which acts energetically and rapidly upon the system, cannot successfully combat such a disease, requires no argument, and yet such a method of treatment is, in fact, very generally adopted, with the result, as some of the following cases will show, of rendering the disease more intractable, while the health of the patients becomes much deteriorated. The rule that an acute disease requires an active remedy, while a chronic disease will only yield to a slow remedy, is generally accepted; there is no disease with which I am acquainted, in the treatment of which this rule should be more carefully borne in mind than in that of syphilis, not only from the essentially chronic character of the disease, but from the fact of the medicine, which possesses the greatest power over its symptoms, and is generally employed in its treatment, being *mercury*, which metal when introduced into the system in too energetic and rapid a manner is capable of doing as much harm, as on the other hand, when used in a cautious and discriminating manner, it is powerful for good. There can, I conceive, be no doubt that by means of mercury the effects of the syphilitic poison in the system may be counteracted to a very great extent, the duration and severity of the disease mitigated, and life saved when vital organs are attacked, and yet the non-mercurialist may boast that his cases on the whole get on quite as well, and some of them very much better than those under the care of practitioners who employ mercury. In an average of a large number of cases treated with and without mercury, I believe there would be found no great difference either in duration or severity of symptoms, and the reason I am convinced is, that on the mercurial side the cases which are improved and relieved by mercury judiciously administered are counteracted by those in which it has been used in too active and energetic a manner, so that if this second general principle is not attended to, it would be much better not to give mercury at all.

Salivation should never be induced in the treatment of syphilis; all the good effects of the drug can be obtained without this, the poisonous effect of the metal upon the system. Not only is the patient injured by the depressing effects of mercury when pushed rapidly on to salivation, but the syphilitic symptoms remain unaffected by this active use of the drug. So that it is always a most unfortunate circumstance should a patient become salivated, and one which we should guard against by, in the first place, using mercury, especially at first, very cautiously, and in small quantities, and also by examining our patients frequently, for some are very susceptible. The best means of giving mercury so as to fulfil the indication of introducing it gradually into the system, and avoiding salivation, is, I believe, by means of inunction; ten or fifteen grains of mercurial ointment carefully rubbed in daily will do more for the relief of the symptoms, than could be credited by those who are accustomed to give it freely by the mouth. Inunction is much the best means to employ in the early stages of the disease, but in the later stages, very often it is necessary to give mercury in combination with iodide of potassium, and then either the biniodide or bichloride may be given in doses of the twelfth of a grain, and the metal will then act in the gradual manner required. The following cases prove the importance of due attention being given to this principle of the gradual introduction of the remedy by which we propose to influence the symptoms of syphilis.

Case I.—*Syphilis in its Second Stage, treated at first by the rapid introduction of Mercury into the system, which, while injuring the patient's health, failed to remove the symptoms—Subsequent gradual treatment, followed by complete removal of the symptoms and restoration of the patient's health.*

J. R., a clerk, came under my observation upon the 3rd of December, 1869. He was then profusely salivated; bloody saliva flowed constantly from his mouth night and day; he could with difficulty swallow even drinks. He was bathed constantly in a cold sweat, his pulse being rapid and weak. The syphilitic symptoms were a papular eruption, well marked on the face, arms, and neck, enlarged cervical glands, pains along both tibiæ, which were worse at night.

He had contracted the disease in August, and the eruption had appeared, he stated, during the following month. He was at this time in the country. The surgeon whom he first consulted gave

him "potash," and he thinks a little mercury, when the eruption and other symptoms became somewhat better. He came to Dublin about business, and then consulted a medical man, who prescribed pills for him, one to be taken three times a day, and after a few days fearing one of his eyes was becoming inflamed, gave one every four hours. At the end of the week salivation was established, with all its attendant evils, while the eruption and other symptoms remained the same as they were before this active treatment was commenced.

When this patient came under my care, the treatment was necessarily entirely directed to the relief of the salivation. Four grains of quinine was given every four hours, while a chlorate of potash gargle was constantly used to wash the mouth; fluid nourishment was given every four hours. In about a fortnight he was recovered, so far as to eat solid food; he then left for the country. I saw him again early in January; he had then quite recovered from the debilitating effects of the salivation, and appeared in good general health. *The syphilitic symptoms remained as well marked as before.* I now considered him in a suitable state for treatment, and commenced at once with the inunction of half a drachm of mercurial ointment once daily, a mixture containing three grains of sulphate of quinine in each dose being given twice a day. After a fortnight's steady perseverance in this treatment the eruption was manifestly decreasing; the inunction was then reduced to fifteen grains daily. Upon the 8th of February, after a month's treatment, the following note was taken:—"Papules scarcely perceptible on the forehead, where they had been very marked; eruption on arms and back nearly disappeared; the papules which remains are scaly; slight enlargement of cervical glands still on one side; no pains or other symptoms; general health very good; has not been a day from business since he returned to town; no symptoms of salivation." By the end of March all the symptoms had completely disappeared, and the treatment was discontinued.

Case II. (Reported by Mr. Arthur Ringland).—*Syphilis in its Tertiary Stage—Salivation produced upon five different occasions without relief to symptoms—Complete removal of symptoms and restoration of general health by gradual treatment.*

A. P., a widow, about thirty years of age, was admitted under the care of Dr. Barton, into the Adelaide Hospital, upon the 12th November, 1869; her condition was then as follows:—An eruption

of both dry and ulcerating tubercles was scattered over the body and limbs; one ulcerating patch upon the back and another near the left wrist-joint, were each as large as a five shilling piece; the surfaces of these ulcers were covered with a dirty semipurulent discharge, their edges red and everted. Several large white cicatrices marked the situations where similar ulcers had healed. Severe pains were felt in both arms, legs, and head; in the latter the intensity was such as to resemble that of severe neuralgia; all these pains were worse at night; the pains along the tibiæ and about the ankles and knees were much increased by standing, so that the patient could scarcely bear to put her feet to the ground. Her hair had fallen to a great extent, and was still falling.

Previous History.—Two years before, her husband had sores on his lip, and she got a similar sore on her upper lip from him; this sore ulcerated, and was followed by a cutaneous eruption, which, commencing on the face and head, spread over the entire body. The throat became very sore about the same time; this was followed by inflammation of both eyes, from the effects of which her sight remains impaired to the present time. When the eruption first appeared she was placed under the influence of mercury, and salivated. She was again salivated, when her eyes became inflamed; when these got better the eruption, pains, sore throat, &c., remained; for these she was given mercury again, and subsequently, in the hands of other medical men, she was salivated twice, so that altogether she has been salivated, during the past two years, five times, and yet she states the symptoms of the disease are as bad now as ever. The patient was emaciated, and had all the appearance of one worn out with loss of sleep, pain, and depraved digestion, and expressed herself as almost despairing of relief.

As the periosteal pains were the most distressing symptoms, and those which deprived her of rest and kept up the irritative fever, her treatment at first consisted in eight grain doses of iodide of potassium, four times in the twenty-four hours, with Dover's powder at bedtime. In about three days there was a decided relief from pain; the mixture was then changed to one containing the twelfth of a grain of biniodide of mercury, and five grains of iodide of potassium in each ounce of decoction of cinchona bark; of this she took one ounce three times a day. She was placed on meat diet with some wine; ordered to take a hot air bath twice a week, and to have the ulcers fumigated with calomel.

Under this treatment the patient steadily improved in every

respect, her general health being restored *pari-passu* with the disappearance of all the syphilitic symptoms. The ulcers soon put on a healthy appearance, but their cicatrization was very slow. After all other symptoms had quite disappeared the ulcer on the back and that on the left wrist still remained. After about five weeks of treatment the patient's improvement was so decided as to attract the attention of all who had seen her when first admitted, and to elicit from herself the remark that she never expected to be so well. About the middle of December, this patient was attacked with shivering and pain in the right side, which proved to be due to diaphragmatic pleuritis, which soon spread to the lungs, and her life was in imminent danger. The right lung becoming solid without at the same time any expectoration, with the assistance of my colleague, Dr. Little, this serious complication was, however, overcome, and the patient made a good recovery. She left for the Convalescent Home, Stillorgan, upon the 21st of January, the syphilitic symptoms having entirely disappeared, and her general health being better than it had been since she contracted the sore upon her lip two years previously.[a]

Case III. (Reported by Mr. Arthur Ringland).—*Tertiary Signs two years and a half after Infection—Salivation without removal of Symptoms—Great General Cachexia—Symptoms removed, and General Health restored by Treatment without Salivation.*

William De C., nineteen years of age, was admitted into hospital upon the 18th December, 1869. He presented the following group of symptoms:—Tonsils and palate bore the cicatrices of former ulceration, and were still red and inflamed looking. Spots of a dark coppery colour were scattered over the face; several cicatrices of rupia existed over the shoulders and back.

Upon the upper and inner part of both legs, near the knees, existed two large ulcers, the one upon the right leg being the largest, and being about the size of a five shilling piece; the edges of these ulcers were not undermined, but the skin around them was of a dark coppery hue; the surfaces were covered with a dirty looking greyish exudation. He complained of nocturnal pains in his

[a] This patient presented herself again at the Hospital in March last, and exhibited two ulcers about the size of a shilling each upon the scalp. Her health was failing; she had been leading a drunken and irregular life. She was admitted, but when she came to the Hospital in the evening she was so drunk as to make it necessary to cancel her admission.

head and legs, so severe as to deprive him altogether of sleep. Upon the top of the glans penis existed the cicatrix of an old sore; the glands in both groins were enlarged and indurated; his tongue was white; there was complete loss of appetite; he was emaciated, and his skin had a yellowish dry look.

Previous History.—About two years and a half before his admission to hospital, he first noticed a single sore upon the glans penis, the mark of which still exists; this was followed shortly by an eruption upon the forehead. He then sought for medical advice; the surgeon whom he consulted cauterized the sore, and subsequently, the eruption having spread all over his body, placed him under the influence of mercury. He became severely salivated, but states that the eruption continued without much change for four months. He now began to drink rather freely, and became infected with gonorrhea; this was followed by his present symptoms of great general debility, and by the appearance of the syphilitic symptoms which he presented upon admission.

The *treatment* adopted was, internally, eight grain doses of iodide of potassium, with the one-sixteenth of a grain of biniodide of mercury, in an ounce of decoction of cinchona bark, three times a day, a mercurial vapour bath every second day, ten grains of calomel being used each time, the ulcers dressed with charpie soaked in black wash; full diet. For several nights after his admission scarcely any sleep was obtained, but he expressed himself always relieved by the baths, and the ulcers began at once to show a more healthy edge and surface. A steady perseverance in the treatment was, however, soon followed by a gradual improvement in all his symptoms; the eruption disappeared; the ulcers healed; the nocturnal pains subsided; and at the same time natural sleep and appetite returned; not the slightest symptoms of salivation appeared. The treatment was continued with but slight alteration until the beginning of February, when being free from all signs of disease, and feeling well, he left the hospital. He has been seen occasionally since that time, and he stated he was free from any symptom such as he had suffered from when admitted.

Case IV. (Reported by Mr. Samuel Knaggs).—*Tertiary Stage—Cerebral Symptoms—Acute Head-ache—Loss of Sight—Loss of Memory—Complete Recovery.*

J. C., a respectable married woman, forty-one years of age, applied at the Adelaide Hospital in October, 1868. She then had

a gumatous tumor on the thigh; four years previously she had contracted syphilis from her husband, and had ever since been suffering from various symptoms of the disease. She had been for several months in Dr. Stevens' Hospital; when better she went out, but subsequently, when the symptoms returned, she placed herself under Mr. Hamilton's care, who salivated her; still her symptoms continued with short intervals of apparent health. She did not come into hospital at this time, but was treated as an out-patient. Upon January the 4th, 1869, she presented herself with a fresh train of symptoms, and was then admitted. She now complained of intense pain of the head, like a tight band; she compared it herself to a feeling as if a red-hot iron circle was tight round her head; both eye-balls were the seat of acute pain; the sight was very defective; flashes of light and *muscæ volitantes* were constantly before her; light could not be endured. Her memory appears to have been defective previously, but it was at this time much worse; recent occurrences seemed to her to have happened "centuries ago;" cannot remember anything; and says she has "spiritual and temporal delusions." The intense pain continued for more than a week after she was admitted, although every expedient, including large doses of iodide of potassium, was used to relieve her; any effort of mind, such even as the attempt to answer a question, increased the pain. Blisters were applied at the back of the neck, and behind the ears, and the twelfth of a grain of biniodide of mercury, with eight grains of iodide of potassium was taken four times daily.

A slow but steady improvement began after this treatment had been in force about two days—first, the pain began to abate in intensity, then the dread of light diminished, then the flashes and muscæ disappeared, and slowly the memory and capacity of mind returned. She remained about two months in hospital, during the whole of which time she continued taking the medicine. She then went to the Convalescent Home for about six weeks, and took the same medicine at intervals, a week being allowed to pass between each repetition of the bottle; all tendency to a return of her symptoms seeming then to have disappeared, at the end of about three months she discontinued the medicine. I have seen her within the last few weeks, and she tells me she has remained perfectly free from any symptoms of her disease whatever, and is able to support herself, filling the situation of housekeeper, but her memory is treacherous and defective.

Those who oppose all mercurial treatment, would find in the early history of each of these cases, ample grounds for the support of their views. We can, I think, hardly doubt that had these patients been treated upon a non-mercurial plan, or not treated at all except by hygienic rules, that they would have suffered less, and that the disease would have run a milder course than was the case under the treatment to which they were subjected. The conclusion, however, which the non-mercurialist would draw, viz.:—that mercury should not be given at all in such cases—does not necessarily follow, for I contend that the evident failure of mercury in these cases, was due, *not* to the wrong remedy being employed, *but* to the right remedy being wrongly employed, and the proof of this is furnished by the subsequent course which each of these same cases ran, where mercury was properly employed.

In the first case, the papular eruption which had continued, in spite of an active salivation, steadily declined, and disappeared under the inunction of fifteen grains of mercurial ointment daily, while the health remained perfect. In the second, an improvement such as caused the patient to say that "she never believed she would be so well," followed the use of the one-sixteenth of a grain of biniodide of mercury, with iodide of potassium, when five salivations and abundance of iodide of potassium alone had entirely failed to give relief.

In the third case, six weeks of the same treatment with mercurial fumigations to the ulcers, sent the patient out of hospital quite free from any symptoms; and in the fourth, in which serious head symptoms existed, similar treatment with counter irritation succeeded, not only in restoring the patient, but having been persevered on for three months, relieved her so much that she remains well after a year has elapsed, having previously been a constant sufferer, although more than once treated by mercury pushed to salivation. I do *not* claim for the mild or gradual method of mercurial treatment which I advocate, the power of eliminating the poison so completely from the system, that no future symptoms will appear, for I have found that patients treated according to the plan I recommend, will frequently show slight symptoms for a year or more. What I do claim for the plan is, that if employed in the early stage of infection, it will remove the symptoms completely, while the patient's health will at the same time improve, and that it will, if pursued for a sufficient length of time, render the future symptoms much milder and of shorter duration than they would otherwise have been.

If the disease is allowed to run its own course, or even if the patient is treated with active mercurialization, the mild mercurial plan cannot be so successfully employed subsequently, but still it remains the best treatment that can be adopted; it will still be able, in most cases, to restore the patient for a time at least to health, and to render the future recurrence of symptoms milder in their character. The methods which I have found the best in carrying out the mild mercurial treatment are—in the early stages, the daily inunction of from fifteen to thirty grains of mercurial ointment, and, in the later stage, either the inunction of ten grains of the ointment, or the mixture of the biniodide of mercury and iodide of potassium, as employed in the cases I have related; with either I occasionally use fumigation, either general or local, according to the nature of the case.

In a certain class of cases, of which the following is an example, fumigation is of much service:—

Case V.—*Tertiary Syphilis—Extensive Ulcers of Integument with Sarcocele—Treatment by Mercurial Vapour Bath.*

W. C., forty years of age, a servant, was admitted into hospital, on the 14th December, 1869. About nine years previously he had contracted a sore which was followed by an eruption on his skin, since which he had been almost constantly suffering from some symptoms of syphilis. His general health was much deteriorated. The left arm from the elbow to the wrist was the seat of several jagged ulcers, oval in shape, about the size of a five shilling piece each, their surfaces were covered with a fetid unhealthy pus, their edges raised and livid. Over the skin upon other parts of the body were scattered the indelible cicatrices of ulcerated tubercles. The right testicle was swollen to about six times its natural size, being heavy, firm, and painless; the left was atrophied.

Pains along the long bones were felt at night. A few indurated glands were to be found in the groin and in the neck.

The treatment which in this case was successful in relieving the pains, healing the ulcers, and restoring the general health in five weeks, was the calomel bath every second day, assisted by eight grain doses of iodide of potassium, in cinchona bark three times a day. No impression was, however, made upon the sarcocele, which remained the same, when the patient left the hospital well in all other respects.

In concluding these remarks, I would ask—How are the conflicting statements of those who advocate, and of those who denounce the use of mercury in syphilis to be accounted for? unless we admit that this medicine may be either useful or injurious according to the manner in which it is used. And again—Why has the external use of mercury been advocated by the most practical and able surgeons as the safest and surest method of treating syphilis in preference to the easier and readier method of internal administration? unless that it was found by experience that the symptoms yielded more completely and satisfactorily, while the general health remained unaffected by the remedy, when employed in this manner. The *gentle* and *gradual* action of mercury was thus gained, while its energetic and poisonous effects were avoided.

ART. XIII.—*On Extraction of Cataract by von Graefe's Peripheral Linear Section.* By HENRY WILSON, F.R.C.S., M.R.I.A.; Assistant Surgeon to St. Mark's Ophthalmic Hospital; late Teacher of Ophthalmoscopy at the Richmond Hospital, &c., &c.

THE subject of cataract has attracted a very large amount of attention from the earliest times, and if we consider how frequently this disease is the cause of blindness, and reflect on the difficulties encountered in its removal, we need not wonder at the very great interest attaching to it, nor at the largely increased amount of consideration and research devoted of late years to the means of its cure. Notwithstanding the ignorance or uncertainty of the ancients as to the true nature and seat of the disease, as is amply testified to both by their writings and nomenclature, many valuable instruments and operations were devised by them for its cure, some of which are retained even still. The needle was employed probably before the Christian era, and in its various modified forms still holds its ground, and is largely made use of; so long ago as the eighth or ninth century a hollow needle for the removal of cataract by suction was in use in Persia, and has been described and figured by Jesu Hali (Isa Ben Ali), a Christian oculist of great repute, living in Bagdad, in the ninth century.

Although we can thus trace back the needle to remote antiquity, the period when the knife, and with it the operation of extraction, was first employed is involved in obscurity. A passage in Pliny

has long led to the belief that extraction was practised in or before his time. The learned Professor Hirsch, of Berlin, has, however, lately brought his profound medico-philological knowledge to bear on the matter, and has given the weight of his authority against this supposition. He adduces evidence to prove that the word *squama* made use of by Pliny does not denote cataract at all, but corresponds to the Greek λεπις, a scale or splinter. A passage from Rhazes (Abu Bekr El-Razi), the Arab Galen of his time (ninth century), has been largely quoted to show that Latyrion, a name which is known only through this one passage, practised extraction. Dr. Hirsch is of opinion that this text does not bear the meaning thus attached to it, and says the assertions made of late years respecting the claims of Latyrion and Antyllus to this operation rest on gratuitous assumption, and copying erroneous quotations without reference to the original. He also remarks that the term *cataract* does not belong to remote antiquity, but that the first mention of it he has found occurs in Platearius a physician living in the twelfth or thirteenth century. Probably one of the first authentic records of extraction is that alluded to by Himly when he mentions that Freytag a surgeon in Zurich removed the cataract through a corneal flap wound in 1694; but so far as I can learn extraction was not practised until the beginning of the last century when the accidental dislocation of the cataract during or after the operation of couching, then much in vogue, necessitated its removal through a corneal incision. Petit, St. Yves and Daviel contributed each their share in bringing the operation of extraction into general use, but to Daviel in particular belongs the merit of devising and perfecting the operation of which he published an account in 1747. Brisseau's previous discoveries or rather demonstrations respecting the position and nature of cataract contributed no doubt largely to the adoption and progress of the new operation. In 1650 Reni Lasnier, a Paris surgeon, maintained that cataract resided in the crystalline lens, and in 1705 Brisseau demonstrated that such was absolutely the case. In the *Philosophical Transactions* for 1724–25, John Ranby the favourite surgeon of King George the Second records the dissection of two cataractous eyes which showed that the lens was the seat of the cataract. In the same volume is contained a Latin communication respecting the eyes of a soldier, who died in the Royal Hospital, Kilmainham, nine years after having been couched in both eyes by Caywood, a Dublin oculist; the eyes were examined by Professor, afterwards Sir Thomas

Molyneux, and no trace of the lens was to be found in either eye. In 1750 O'Halloran one of the most eminent surgeons of his day and practising in Limerick, published a treatise on the cataract prefaced by the significant sentence from Seneca, *veniet tempus quo, ista quæ nunc latent, in lucem dies extrahet;* in this philosophical essay the author shows the cataract to be in the lens. O'Halloran was amongst the first British surgeons to extract the cataract as is shown by his essay in the second volume of the *Transactions of the Royal Irish Academy* published in 1788; he advocated a corneo-sclerotic incision somewhat similar to that of von Graefe to which I have to refer, and suggested also the removal of the cataract in its capsule. Some of the earliest writings in the English language on the extraction of cataract are those afforded by Warner 1775, Chandler, Butter, &c. This operation has gradually been perfected and has now, I rejoice to say, superseded in all civilized countries the dangerous and reprehensible practice of couching or reclination—dangerous because even if temporarily successful the depressed lens having been forced through and broken up the vitreous humour and hyaloid membrane is placed lying in the bottom of the eye in contact with the retina, and pressing upon it, the choroid and ciliary body; in this new position it acts as a foreign body giving rise constantly to a slow insidious form of inflammation or even panophthalmitis, which destroys not only the sight of the operated eye, but also frequently that of the second eye through sympathetic extension of the irritation. Couching cannot be too strongly or too loudly condemned, and if mentioned at all in writing or teaching it should be alluded to solely as a matter of past history.

The operation of extraction has undergone various modifications and improvements; some authorities, such as O'Halloran, Butter, Earle, Benjamn Bell, as well as continental authorities recommending the sclerotic or the corneo-sclerotic junction as the best place for the incision, the majority of operators advising the cut to be made entirely in the transparent cornea; some made the section above, others below and again others at the side of the globe. Most operators made a large incision, some such as Wardrop, Travers, and Adams made a small section, Wardrop recommending an opening one quarter the periphery of the cornea. The operation for the removal of senile hard cataract as eventually practised, and which held undisputed sway amongst scientific men down to a few years ago, is known as the flap extraction, and is performed by making an opening in the transparent cornea to the extent of one-

half of its circumference, opening the capsule of the lens and expressing the cataract through the pupil and through the corneal opening. In cases of soft cataract in juvenile persons, Mr. Gibson of Manchester first broke up the lens with a needle, and after some time made a small straight or linear incision in the cornea through which he removed the broken up lens with a curette. Mr. Walker, also of Manchester, devised the grooved needle-knife here figured,

Fig. 1.

and which was originally described by Sir Wm. Wilde in this journal and by him introduced into practice. In recent traumatic cataracts the lens is quite soft and will readily flow out through the groove when the instrument is pushed through the cornea into the lens.

Flap extraction is amongst the most beautiful of operations, and yields when successful the most perfect results both as regard vision and appearances; cases occur in which it is difficult to recognize any traces of an operation; it requires however great steadiness and quiescence on the part of the patient, and it demands on the part of the operator considerable manual dexterity (only to be acquired by practice), keen vision, calmness and control of temper, and patience which the most untoward events will not ruffle or exhaust; a correct judgment and promptitude in action which will enable him to encounter successfully the various casualties which are likely to occur; he should possess moreover an extended knowledge of human character in general and of his patient in particular; and in addition to this list of personal qualifications he must be provided with an experienced assistant who should act with him in unison of mind and body, who should in fact be to him a third hand. By constant practice and long experience an operator may acquire such expertness that the operation becomes in his hands almost a certainty; but no dexterity and no experience can avert the sometimes disastrous consequences of the operation; the real dangers commence only after the completion of this operation, then ensue the long and weary days and nights of careful watching, and of alternating hopes and fears, a most anxious time for both patient and attendant, as I well know from my past experience. I will now very briefly refer to some of the chief difficulties and dangers during and after flap extractions, in even the most skilful hands.

There are certain measurements and indications laid down for the extent and position of the incision in the cornea, but it is found impracticable, except under chloroform and with fixation of the globe to make a perfect section in every case, it may result too small or too large, its position may be faulty or it may have to be completed or enlarged with the scissors or knife; the iris may be cut, vitreous may be lost, hemorrhage may occur; I have even seen the whole contents of the eye, iris, lens, vitreous, retina, and choroid projected through the wound. In order to express the cataract in the normal extraction the lens must be rotated to a great extent in its axis and pressed through the pupil; now by these acts considerable pressure must of necessity be exerted by the lens on the iris and vitreous humour, and in consequence of this bruising of the iris inflammatory changes are very apt to occur in that structure. In cataracts with soft periphery the soft matter is very likely to be scraped off and to remain behind the iris where it gives rise to iritic complication and neoplastic formations in the pupil. In some instances the vitreous is perfectly fluid, and the lens when freed from its capsule falls down into the bottom of the eye, from whence it is sometimes almost impossible to remove it. Another great difficulty in flap extraction is presented by adhesions of the iris to the lens. Of course unsteadiness on the patient's part and spasm of the ocular muscles will greatly complicate the difficulties of the operation. I have more than once seen the operation attempted and postponed to another day in hopes of the patient becoming more quiescent. The principal dangers which retard or prevent the successful recovery are non-union or reopening of the wound generally with and rarely without prolapsus of the iris, ulceration or sloughing of the edges of the wound, suppuration of the cornea, iritis, chorido-iritis, panophthalmitis; hemorrhage is of very rare occurrence after flap extraction, it does however sometimes occur, and if to any great extent results in loss of vision. It is but natural to suppose that a wound of such magnitude cannot be made with impunity in a non-vascular structure, like the cornea in persons advanced in life; owing to the great extent of the cut the cornea is peculiarly liable to destructive changes. It is a matter of great importance that the surface and edges of the wound should be in perfect apposition and heal by primary intention; practically it is found very difficult to keep the opposed lips of the wound in perfect co-aption; hence Dr. Williams, of Boston, has recently recommended the wound to be united by sutures; he states he has

obtained most satisfactory results from this practice. Suppuration of the eyeball commencing either in the cornea or in the uveal tract is of all the sequences the most fatal to the eye; and occurs even under the most favourable circumstances in about 7 per cent.; iritic complications are of very constant occurence and result in complete or partial closure of the pupil as also in neoplastic formations in it requiring secondary operations; non-union or reopening of the wound with prolapse of the iris retards the recovery very much and results in distorted pupil and large cicatrix of wound. Loss of vitreous during flap extraction results generally though not invariably in a broad and extensive corneal cicatrix and horse-shoe shaped pupil. Secondary hemorrhage, vomiting and diarrhea are amongst the rare sequences. Although flap extraction yields when successful as I have stated the most perfect results, results such as no other operation has attained, yet the dangerous sequences are so great and so many, that operators have been induced to seek for some more safe method of removing the cataract. About the year 1861 von Graefe, of Berlin, occasionally performed an iridectomy in cases where there had been any difficulty in expressing the lens or in which he had reason to anticipate an unfavourable recovery. A couple of years subsequently one of von Graefe's assistants, Dr. Schuft, now named Waldau, removed the lens by means of a spoon-like instrument through a comparatively small straight incision in the transparent cornea. It is right to mention, however, inasmuch as the spoon has become a generally adopted instrument that von Graefe claims having made use of such an instrument long before Waldau, and states correctly that after all the spoon is merely a modification of Daviel's instrument so long and still in use. Figs. 6 and 7 on plate 8 of Scultet's *l'Arcenal de Chirurgie*, 1675, might almost be taken as the prototype of the curette and spoon. The idea of removing the lens by an instrument introduced through the pupil to the back of the lens is not new, for Bell in his *System of Surgery*, 1786, recommends a hook for that purpose. Butter made an incision into the sclerotic with an instrument which opened forceps-like, seized and withdrew the cataract. To avoid bruising the iris and to avert subsequent inflammation as well as to facilitate the exit of the lens iridectomy afterwards became a feature of Waldau's scoop operation; the spoon underwent various modifications and improvements, Mr. Bowman's being I think the best, and eventually an operation known as the traction extraction of Bowman and Critchett came into general use. This operation

consists in making a straight incision into the transparent cornea immediately at its junction with the sclerotic, or better into the corneo-sclerotic junction itself, excising a segment of the iris, opening the capsule and then introducing the spoon between the lens and the vitreous and lifting out the cataract in it; the cut is made at the upper part of the globe by means of a triangular spear-shaped Jaeger's knife, bent on the flat and embraces nearly one-third of the corneal circumference. In consequence of the cut being made superiorly, the iridectomy, which is a necessary portion of the operation, is concealed by the upper lid—indeed as a rule I invariably perform iridectomy when undertaken for the relief or cure of glaucoma or in cataract operations at the upper portion of the iris, because the upper eyelid not only conceals the coloboma which might be considered unsightly, but also because it cuts off the peripheral rays of light which might prove dazzling and troublesome. Although some of my most successful cases eventuated from traction extraction I have discontinued the operation as it did not yield me the satisfactory results I had anticipated; I have had suppuration after it both in private and in hospital practice. I may mention incidentally that in one of these cases of suppuration which occurred in my practice at the Richmond Hospital I subsequently removed the cataract from the second eye by the old flap extraction with the most satisfactory termination. I regard the introduction of traction instruments behind the lens as a dangerous procedure more especially in elderly persons with weak fatty heart; such persons are indeed I think peculiary liable to suppurative inflammation after any extraction. Iridectomy has now become an almost indispensible feature of extraction no matter by what method; Mooren of Düsseldorf was in the habit of performing iridectomy some weeks prior to the flap extraction, but notwithstanding the very satisfactory results he obtained (only 14 losses out of 229 extractions), he has now abandoned his method for that of von Graefe. Removal of the lens in its capsule which I have referred to was practised, occasionally by Beer, Richter, and some of their pupils; Dr. Pagenstecher, of Wiesbaden, who has revived this procedure makes an iridectomy downwards. Jacobson executed a flap extraction with iridectomy following the expulsion of the lens—a proceeding I witnessed occasionally in von Graefe's clinique several years ago. Professor Macnamara, of Calcutta, after dilating the pupil with atropine and chloroforming the patient makes a straight incision in the temporal

side of the transparent cornea to the extent of half an inch, and while withdrawing the knife opens with its point the capsule of the lens; he then introduces a fenestrated spoon by which pressure is made on the edge of the lens, which causes the latter to tilt over in its axis and to lie in the scoop. I have not performed this operation, which appears to me very similar to Schuft's original scoop extraction, but I should fancy that the loss of aqueous humour and the irritation of the operation would tend in a large number of cases to contract the pupil, and that there must of necessity be a good deal of bruising during the manipulations; the professor states, however, that he obtains perfect results where the flap extraction utterly failed. I now come to the latest modification of extraction from which I have been led into this long digression by the great interest attaching to the history and literature of extraction in general.

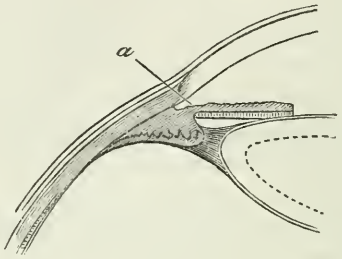
This modification, which is known as von Graefe's extraction by peripheral linear section, or modified linear extraction has been adopted by many operators, and as it appears to answer the requisite purposes, and seems to me to afford a safer means of removing cataract than the flap extraction, I would wish to bring it under the notice of the members of the Irish profession, who may have few opportunities in country districts, and little leisure for consulting special works and periodicals. It is due to Dr. Taylor, of Nottingham, to mention here that quite independently of von Graefe he adopted in isolated cases a section almost identical with that I am about to describe. Dr. Taylor performed prior to the section what seems to me a very small and incomplete iridectomy, as he made it through such a small incision in the cornea as was "just large enough to admit a small blunt hook." His notice is contained in the April number for 1866, of the *Ophthalmic Review*, a publication which has I regret to say ceased to exist.

Von Graefe endeavours to avoid the principal dangers of flap extraction, by making the section as small as possible compatible with the exit of the lens, in such a structure as is least liable to suppurative inflammation, and in such a position and manner as will allow of the most facile delivery of the lens and of the most perfect coaption of the surfaces of the wound, as well as by adopting such precautionary measures as are likely to guard against subsequent destructive inflammation. These objects he attains, I conceive, by executing a small nearly straight section in the sclerotic border, and corneosclerotic junction, nearly opposite the

margin of the lens, by making a conjunctival flap which shall cover the wound, and by performing an iridectomy.

The accompanying cut copied from von Graefe's essay in the

Fig. 2.



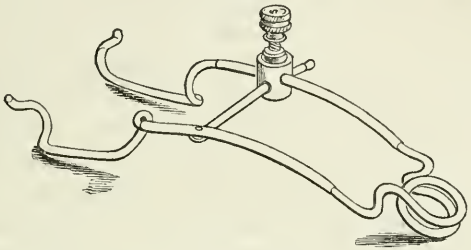
Archiv für Ophthalmologie for 1865, illustrates the relative position of the parts, the line *a* representing the incision. It is well known that wounds in the sclerotic and corneo-sclerotic junction are less liable to suppurative inflammation, and that they heal more readily than those in the transparent cornea; the external wound measures $4\frac{1}{2}'''$—$5'''$[a] and the internal one $4\frac{1}{2}'''$, the height of the flap does not exceed $\frac{1}{4}'''$ whereas it measured $2'''$—$2\frac{1}{2}'''$ in the old flap extraction. The wound in von Graefe's operation, though strictly not a linear one, may for all practical purposes be regarded as such; its surfaces lie in exact apposition, it is covered over by the conjunctiva, and is in every respect in the most favourable position for immediate union. By a reference to the illustration it will be seen that the opening is as nearly opposite the margin of the lens as is possible without interfering with the ciliary body and processes. The manifest advantage of this position as regards the delivery of the lens, is that when the iris is removed, the capsule opened and pressure made at the opposite side of the globe, the lens glides easily and naturally into and through the wound; the disadvantage is that the internal corneal margin being cut across the zonula is thereby deprived of its support, and there arises a tendency towards escape of vitreous, a circumstance which has occurred in about twenty-five per cent. of the cases I have operated on.

The operation is performed as follows:—The patient is placed in the recumbent position, on a couch or bed opposite a good light, the eye not to be operated on is covered or bandaged

[a] The foot is expressed by (′), the inch by (″), and the line by (‴).

down, the eyelids separated by the spring speculum here figured and the globe drawn downwards by a toothed forceps applied

Fig. 3.

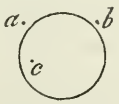


immediately below the inferior margin of the cornea; in fixing the eyeball a broad fold including the sub-conjunctival cellular tissue and even the insertion of the inferior rectus if feasible should be grasped between the blades of the forceps, for the conjunctiva alone affords often but a slight hold and is liable to be torn through. In fixing the globe it should be rotated directly downwards, otherwise the section and consequently the iridectomy will fall towards one side and the resulting coloboma will be exposed to view and appear unsightly as well as give rise possibly to disturbing dazzling. When the operator has thus steadied the eyeball he holds the knife here figured in his other hand, its flat surface parallel to the plane of the iris, its cutting edge directed upwards

Fig. 4.

and inwards, and inserts its point into the sclerotic $\frac{1}{2}'''$ from the corneal margin at *b*, Fig. 5, copied from v. Graefe, which represents

Fig. 5.



the left eye, and pushes the instrument obliquely downwards and inwards into the periphery of the anterior chamber towards *c*, in order to secure a large internal wound. When the knife has entered the chamber in this direction, to the extent of $3'''$ or $3\frac{1}{2}'''$ its handle is depressed and the point thus elevated is directed

towards the counter puncture *a*, and pushed onwards beneath the opaque cornea into and through the sclerotic border. When the point of the knife has penetrated the sclerotic and appears either underneath or outside the conjunctiva, the cutting edge of the instrument should be turned very slightly forwards, and the section completed in the corneo-sclerotic junction, either by a sawing motion or an onward thrust. The blade of the knife then lies free immediately underneath the conjunctiva, and should be still further rotated with its cutting edge forwards or even forwards and downwards and the conjunctiva divided in this direction. By this means a conjunctival flap of not too large dimensions is formed which subsequently covers the wound. In making this section the knife should be entered at the extreme periphery of the anterior chamber, and should leave it in a similar position; care must however be taken not to introduce the knife too far downwards and inwards into the chamber before reaching the counter-punctuation and not to direct the point of the knife too far backwards, otherwise a large gaping wound will be made in the sclerotic implicating the ciliary body and exposing the vitreous humour, neither should the counter-puncture be in the transparent cornea; if a faulty puncture or counter-puncture be made the point of the knife should be withdrawn and inserted in the desired position. The outer wound is almost altogether in the true sclerotic tissue while the inner is in the corneo-sclerotic junction. It happens sometimes that the point of the knife after dividing the sclerotic pushes the conjunctiva before it instead of piercing it and the aqueous humour escapes into the bag of the conjunctiva; this circumstance should not alarm us, nor prevent the section being quietly completed, but when the knife has been cut out the conjunctival opening should be enlarged with a scissors down to the point of counter-punctuation. With the completion of the section terminates the first step of the operation; the forceps is then given to the assistant who draws down the globe and the operator turns the conjunctival flap back over the cornea. The prolapsing iris being thus fully exposed is seized in the iridectomy forceps near one angle of the wound drawn but very slightly forwards and cut through with a straight or angular scissors as close as possible to the corner of the wound; its ciliary attachment is then freed by a touch of the scissors or torn through by drawing the forceps with the iris in it to the opposite angle, where it should be divided also as close as possible to the corner of the wound. In quiescent patients with moderately

prominent eyes I sometimes make the iridectomy without having the globe fixed. It is very important to remove the iris up to its ciliary attachment as completely as possible, especially at the angles, for otherwise it is very likely to prolapse, retard the healing of the wound, and give rise subsequently to a cystoid cicatrix, which is peculiarly liable to become irritable. It requires some little care also to avoid cutting the conjunctiva with the scissors, it should therefore be carefully turned down over the cornea before the iridectomy. On completion of this second step the globe is released by the assistant, and if there be no complication the third step that of opening the capsule is to be forthwith proceeded with. If there be hemorrhage into the anterior chamber from the iris or conjunctiva it should be removed before lacerating the capsule, gentle upward movements of a spoon or curette over the front of the cornea and slightly opening the wound will generally suffice to remove the blood. The laceration of the capsule seems to me the most difficult part of the whole operation and requires considerable dexterity and judgment. The operator fixes the eye, introduces the cystotome on the flat as far as the inferior border of the pupil where he gives the instrument a half turn, whereby its cutting sickle-shaped edge comes into contact with the convexity of the lens, and in this position he draws the instrument upwards and thus opens the capsule; it is well to make two cuts starting from one and the same point inferiorly—the one directed upwards and inwards, the other upwards and outwards; an incision along the periphery corresponding to the section is also advisable. In this manipulation great care is to be taken not to displace the lens—a thing difficult to avoid in old cataracts with tough degenerated capsules. If the lens be accidentally disturbed it should be replaced *in situ* by a hook or curette, otherwise its faulty presentation to the inner wound will cause difficulty in its delivery. If there be adhesions of the iris to the lens they should be divided with the cystotome. The fourth and last step consists in the expulsion of the cataract; the fixation forceps is shifted to one side of its former position, the convex smooth surface of a caoutchouc or tortoise-shell spoon which will not injure the corneal epithelium is placed on the lowest part of the cornea and pressed against it in a backward and upward direction; this pressure causes the lens to start and should be continued steadily backwards until the lens is fairly in the wound, when the pressure should be directed upwards by gliding the spoon along the cornea, following the movements of

the lens until the whole cataract has been expelled; should portions of the soft cortex be stripped off and remain in the pupil they are to be pressed out by the action of the spoon on the cornea and by opening the wound. If there be any difficulty in expressing the lens the posterior lip of the wound may be slightly depressed while the pressure is continued below—an assistant fixing the eye; it is advisable at this stage of the operation however if fixation be necessary for the operator to control it himself, as improper pressure might easily cause loss of vitreous. In the majority of cases of ordinary cataracts this pressure properly directed will suffice to express the lens; in some instances however notably where there has been much loss of vitreous or where the vitreous is fluid it fails, and we must then have recourse to traction instruments, such as hooks or spoons; v. Graefe's hook bent in the stem so as to adapt itself to the convex posterior surface of the lens is introduced on the flat, given a half turn and inserted into or beneath the lens and the latter withdrawn, or a spoon is made use of as in the traction extraction. Where the vitreous is known to be fluid we should remove the lens with the hook without having recourse to pressure. Before bandaging the eye we should satisfy ourselves as fully as possible by testing the vision and otherwise that all portions of the lens have been removed: after taking the speculum away and allowing some time to elapse we should gently rub the upper lid over the globe by which means often small particles which had previously remained concealed behind the iris will come into view; these as well as pieces of capsule should be carefully removed as they are, if left behind, most likely to cause iritic complications and neoplastic formations; the time thus spent no matter how long should not be grudged or considered illspent, for on the *complete* removal of the lens depends in a great measure the ultimate success. I am in the habit of testing the vision at this stage; in the ordinary cases where a cataract has been perfectly removed the patient should recognize and distinguish faces. All filaments of blood and particles of lens are to be removed from the wound by means of a curette. In one instance of normal operation in which vision was as perfect as possible under the circumstances a small clot of blood remained at the inner angle of the wound, and with a view of removing it I seized it with the iridectomy forceps as I had often done previously; at the very moment however the patient made a sudden movement with her head and I saw the iris stretching and, before I had time to open the forceps, the ciliary attachment at its

lower and inner side gave way and blood poured forth which quickly filled the anterior chamber; the reason of this was the fact that I had unawares seized in the forceps a piece of prolapsing iris which had been concealed by the clot of blood. I may mention that notwithstanding this serious and avoidable accident the patient after some months recovered very useful sight. The error I committed is instructive in several particulars—but more especially in showing how careful we should be to remove the iris completely at the angles of the wound; it shows also perhaps the necessity for having the patient's head held and steadied by an assistant during delicate operations on the eye; as a rule I do not adopt this practice in extraction preferring to leave it to the patient's own sense not to stir, neither do I as a rule allow the patient to hold the hands of bystanders as I think it conduces generally to spasmodic movements which would otherwise not take place; the trust and confidence reposed in the patient are usually responded to by quiescence. When the operation has been completed a piece of fine linen or muslin about $5'' \times 3''$ divided up the centre to the extent of about $2''$ is placed over the eyes the undivided portion on the forehead the division being occupied by the nose, the hollows around the eye especially at the inner angle are then carefully filled up over the linen by cotton wool and the cotton packed on so that its surface shall project beyond the level of the orbital margin and nose; over this is placed Liebreich's bandage and tied pretty tightly. By this means a uniform and equable pressure is kept up on and around the eyeball, and the bandage is so constructed that no movements of the patient's head can displace it, while it admits of being opened and reapplied and of the dressings being changed without disturbing the patient.

This modified linear extraction is not difficult of execution in full ordinarily prominent eyes such as are met with in Germany and in England; in Ireland however the eye is, particularly amongst the native Celtic population, generally small and very deep-set with prominent overhanging eyebrows, and these circumstances render the operation often extremely difficult; if a line be drawn from the eyebrow to the lower orbital margin and another from this line to the cornea the latter one measures often $6'''$, in one instance of flap extraction recorded in my notes it measured as much as one English inch, in this case the space from angle to angle measured not quite $\frac{3}{4}''$ and the natural opening of eyelids $\frac{1}{4}''$; these measurements which though not common are yet

not exceptional, illustrate one of the difficulties we have to contend with in eye operations in this country, and explain the smallness of Sir Wm. Wilde's flap extraction knives, the cutting edge of which is often only $\frac{3}{4}$ of an English inch or 9‴ long, the whole blade being 1 inch, while the shortest of the knives purchased by me in Germany measures 16‴ in the blade, and 13‴ in the cutting edge, the longest 18‴ in the blade and 15‴ in the cutting edge; such instruments as these latter would I need scarcely remark be quite unsuited to the generality of eyes in this country. In cases such as I have alluded to no matter how far we may rotate the eye downwards the very prominent eyebrow interferes with the execution of v. Graefe's section, and I have been endeavouring to obtain some curved or angular modification of v. Graefe's knife to meet the exigencies of the cases. Another difficulty is presented in the speculum; the best form for all purposes is that already figured which is the one for the left eye, that used for the right side having the regulating nut on the opposite arm; this nut should in v. Graefe's operation be always next the lower lid so as to be out of the way of the knife; a good practical speculum for this new extraction is still a desideratum. I have employed the rack screw speculum of Mr. Noyes, of New York, but have not found it satisfactory as it is apt to slip out, perhaps at the most critical part of the operation. The blade of v. Graefe's knife represented in Fig. 4, the natural size should be as narrow as possible compatible with solidity and strength, its point and edge should be very sharp; in sharpening instruments cutlers are apt to lessen their size and strength, and this knife is very likely to suffer in the process; in one case I had some difficulty in making the section from the fact of my knife being thus thinned and weakened. In the ordinary knives the handle where it joins the blade is usually cut sharply off nearly at right angles, after lowering the handle and pushing the instrument onwards towards its counter-puncture it has been more than once completely arrested in my hand by being caught at this part of the handle by the crossbar of the speculum; I therefore have had this abrupt termination altered and the handle tapered off as shown in the woodcut. It is advisable also to have some mark on the handle by which to distinguish readily the cutting edge from the back of the blade; it has happened that the instrument has been introduced into the eye with its cutting edge directed downwards and outwards instead of upwards and inwards. The speculum requires some

little attention from the assistant, thus it should not be allowed to press upon the globe, and its crossbar should be watched so as not to interfere with the play of the knife.

I must leave the question concerning the advisability of chloroform administration to the individual operator; if employed complete anesthesia should be produced, otherwise the unconscious struggling and muscular spasms during incomplete anesthesia will be worse than the unsteadiness of the waking and conscious individual. Chloroform renders the majority of operations much easier of execution and ensures probably in delicate eye operations greater accuracy and possibly therefore more perfect results. From my early training in St. Mark's Hospital and in Vienna I have acquired the habit of dispensing with the use of chloroform in non-painless and indeed in most eye operations and have not given it in any single case of v. Graefe's extraction. Although I have not as yet employed chloroform in this extraction I do not condemn its occasional use nor would I hesitate to use it were I pressed to do so and there were no contra-indicating circumstances; beginners and unsteady hands will find chloroform of the greatest assistance.

I do not adopt any preparatory treatment, and prefer operating as soon as possible after informing the patient of the advisability of an operation; in hospital it is desirable to operate at once for many reasons, not the least cogent of which is the fact of the patient being sometimes terrified by the alarming and often absurdly false statements made to him respecting operations by patients already in the hospital. I have not unfrequently traced the nervousness and unsteadiness during an operation to such sources. When practicable I operate on the day of admission or the day after, and as a rule only on one eye at a time. I have operated on the second in a week after the first, but two to three weeks at least should I think intervene between the two operations. I perform all cataract operations with the patient in the recumbent position I myself standing behind and above that head, being ambidexter I use the knife, needle, or forceps with the right or left hand indifferently according to the side to be operated on. I prefer the standing position as I thereby obtain greater command of view and of action than in the sitting posture; there are also some advantages I think in being ambidexter and I would advise all beginners to practice eye operations on the subject with both hands. During this as well as flap extraction it is well

to divert the patient's attention from the operation by engaging him in conversation on some interesting or to him familiar topic.

In the after treatment we must be guided by circumstances. I always have the patient placed in bed immediately after the operation; the first night I almost always give a full opiate, and throughout the treatment I order a generous dietary, with wine, brandy, or porter; the patient should not be restricted to any one position, but may be allowed to lie in the most convenient or usual posture; he may be allowed to sit up in bed on the second day and out of bed on the third or fourth day if the hospital arrangements are suitable. The bandage should be opened and the linen changed at least once a day, if there be much discharge from the eye it may be necessary to change it more frequently—but it is desirable to have the eye perfectly quiescent for the first twenty-four hours after the operation; I am in the habit of instilling atropia solution on the second day after the operation, and if any iritic complications exist earlier and several times a day. The compress bandage should be kept on for at least three days, the pressure being somewhat relaxed on the second day after the operation, unless in cases of sanguineous effusion or threatened suppurative inflammation. The bandage should be dispensed with gradually and it is well to guard against any possible accidents by replacing it at night during the whole of the first week. In the day-time the light in the apartment is to be moderated and the patient should wear coloured glasses for a fortnight at least after the operation, too early exposure of the eye is often punished by inflammatory symptoms. I have in very rare instances had recourse to depletion by leeching or cupping,—when inflammatory processes did arise I contented myself for the most part with the frequent instillation of atropia, the continuance or the relaxation of pressure, the application to the brow of an ointment composed of 1 drachm of extract of belladonna and 3 drachms of mercurial ointment and the administration of suitable internal remedies such as purgatives, tonics, stimulants, alteratives and absorbents, and hitherto my results have been quite satisfactory. I have had a few instances of hemorrhage occurring some days after the operation—in one a couple of hours after leaving off the bandage; when blood is effused into the anterior chamber the compress bandage should be retained firmly applied until the blood has been absorbed. Slight serous chemosis is of not uncommon occurrence but so far as my observations extend is not by any means a

dangerous symptom; it has occurred generally without any swelling or discoloration of the lids and appears often due to the sub-conjunctival section and the escape of the newly-formed aqueous humour beneath the conjunctiva; in most of those cases the chemosis was confined to the lower part of the globe; the treatment consists in aggravated cases in snipping one or more pieces out of the raised conjunctiva with a curved scissors and allowing exit to the fluid.

Fig. 6.

The accompanying cut shows the condition of the eyes after the operation in a man aged 69. In order to illustrate the peculiarities of the operation fully I have drawn the right eye in its normal position, the left with the lid raised so as to expose the whole iridectomy as well as the track of the wound, indicated by the black line above the cornea; at the time the drawing was taken the right eye had been operated on a month, the left a fortnight; a small portion of the capsule was opaque in the left pupil and there was a good deal of vasularity about the eyeball; the iridectomy, as may be perceived, is rather large, but this is a fault on the right side; the patient presented somewhat sunken eyes and the shaggy eyebrows hung down over the eyes concealing the eyelids; this latter condition together with the frown peculiar to cataractous persons which was so marked in this individual is well rendered on the right side in the engraving which has been admirably executed by Mr. Hanlon.

The average length of time spent by the patients in the hospital from the date of operation until dismissal was 21 days, the shortest period having been 6 days, the longest 62 days; the long periods refer to patients who had both their eyes operated on. These figures contrast most favourably with those of flap extraction in the same hospital, the average number of days for which was 43 and has reference only to one eye—the second having as a rule never been operated on during the same visit to the hospital.

The apparent immunity from the dangerous consequences of flap extraction has led me to execute the peripheral linear extraction even in the most hopeless cases which formerly I would not have interfered with, and hitherto with the most satisfactory results quoad the mere operation and the recovery of the eye from its effects. Wherever there exists the faintest chance of improvement or where urged by the patient, who may have come a very long distance in expectation of an operation, I never hesitate now to give him the benefit of the doubt, and up to the present I have no cause to regret having done so in hospital practice, on the contrary the operation has in a few instances benefited the individual contrary to expectation. I have also used this operation with advantage in cases affected with granular conjunctivitis, a disease which is so prevalent in this country that it is practically impossible under present arrangements to exclude it altogether from the hospital. I entertain very decided views as to its contagious nature and consider that patients affected with it should be rigorously excluded from contact with individuals suffering under other ocular diseases.

This new extraction is also applicable to immature cataract and to cataracts with adherent pupil; one of its great advantages is its applicability to cataracts in middle aged persons or even youthful individuals; I have operated by this method on a few below 30 years of age and in one instance on a boy 11 years old affected with traumatic cholesterine cataract; the youngest age at which flap extraction is recorded in my notes as having been performed at St. Mark's Hospital is 38. The linear peripheral section may also be employed in other operations with advantage.

The crowning feature of this new method of extracting consists in the very rare occurrence of suppuration; as yet there has not been a single loss from this cause in the hospital; the only case of suppuration I have had occurred in my private practice in a gentleman whose other eye had been unsuccessfully operated on elsewhere some years previously.[a] Contrasting v. Graefe's new modified linear extraction with the old flap extraction it has I conceive the following advantages: it makes less demands on the volition and quiescence of the patient and requires neither the great experience nor the manual dexterity on the part of the operator nor the trained assistance requisite for flap extraction; there are fewer and

[a] Since writing the foregoing I have had to deplore the first and only loss by suppuration which has occurred in sixty cases of the operation at St. Mark's Hospital.

less grave casualties during the operation, and there is no difficulty with the iris during or after the operation; the size and position of the wound are matters of almost certainty; the lens can be always removed no matter how fluid the vitreous may be; the wound heals readily, there is great immunity from suppuration, the time occupied in the recovery is diminished by one-half, the after treatment is exceedingly simple, the operation is applicable to a greater number and larger variety of cases than the flap extraction. I may mention that several friends and pupils to whom I have shown and explained this operation and who then tried it themselves speak in the highest terms of the satisfactory results they have obtained by its means. As is well known it is necessary to substitute a glass lens externally for the cataractous lens we have removed; the focus of this cataract glass for distant objects is after flap extraction as a rule about $3\frac{1}{2}''$, and for reading from $1''$ to $2\frac{1}{2}''$; in this new linear extraction a much lower powered glass answers, thus $4\frac{1}{2}''$ and $5''$ are very common walking glasses and some of my patients get on very well without them altogether; whether this be any advantage I cannot at present say, it shows however that this new operation does not alter the corneal curvature to the same extent as is done in the flap extraction.

In conclusion I believe that in this new operation we have a much safer and more speedy means of curing cataract than in any method hitherto practised, and one which is particularly applicable to hospital practice. I do not however advocate its adoption to the entire exclusion of flap extraction which may still be retained with or without iridectomy in some cases especially in private practice.

ART. XIV.—*Transfusion Successful in a Case of Post Partum Hemorrhage.* By THOMAS E. BEATTY, M.D., M.R.I.A.; M.D., T.C.D., Hon. Causa; Ex-President of the King and Queen's College of Physicians in Ireland; Ex-Professor of Midwifery, and formerly Professor of Medical Jurisprudence, Royal College of Surgeons in Ireland.[a]

MRS. ———, aged twenty-four years, a very handsome, tall, well-made lady, had a premature confinement of her first child at the fifth month, in January, 1867. She became again pregnant, and was delivered of a large healthy boy, at the full time, in July,

[a] Read at Obstetrical Society April 9th, 1870.

1868. She nursed her child for nine months, and was again pregnant in September, 1869. Towards the end of November of that year, she had discharge of blood from the vagina, without pain, which came on every night, and lasted till morning, in small amount, and then disappeared during the day. She was kept in bed, and directed to apply vinegar and water on her napkins, and to take five grains of gallic acid two or three times during the night, as required. Sometimes two or three nights would pass over without any bleeding, and then for a few nights it would return. In January of the present year the intervals became longer, but still there were occasional returns, which were always checked by the gallic acid and cold applications. In this way the month was passed. She became larger, and the uterus could be felt above the pubes. A distinct placental souffle could be heard about the middle of the mouth, but the fetal heart was not audible. She was now allowed to get on a sofa in her room during the day, and there was no further hemorrhage. On the 18th of February, finding herself so well, she went to her drawing-room, where she still observed the recumbent posture, and did the same for four days.

On the morning of the 22nd of February I was sent for at 7 o'clock, and on my arrival I found that she had been seized with labour an hour before, accompanied with profuse hemorrhage. She had no nurse; her mother was alone with her. A six month child was expelled, alive, and lying in an ocean of blood. The patient was pale, pulseless, and cold as marble. The placenta was still in the uterus. Upon making pressure on the abdomen, a very large coagulum of blood was expelled from the vagina; the uterus felt firm; and on tracing the cord up, the os uteri was found closed to the size of a two-shilling piece, holding the placenta within. A slight draining of blood continued to flow. The only stimulant at hand was a bottle of sherry, of which I gave her three glasses in rapid succession. It was very plain that she was in imminent peril, and I despatched a messenger to summon Dr. Denham, who came to my assistance without delay. Before his arrival, I gave her a drachm of ergot in more wine, and had jars of hot water applied to the feet. On consultation with Dr. Denham, we agreed, that as there was still a draining of blood going on, it was necessary to remove the placenta. This I requested him to do, as his hand was smaller than mine, and would more easily enter the uterus, which was only developed to the sixth month of pregnancy, and the os uteri was closed upon the umbilical cord. With very

considerable difficulty this was accomplished, while I took charge of the uterus above, and made pressure to insure its contraction. A quart of vinegar and water was thrown up the vagina with a syringe, and all bleeding then ceased. Our endeavours were now directed to restore the exhausted vital energy of our patient. A pint of warm beef-tea and a glass of brandy were thrown up the rectum, and hot brandy and water were given by the mouth freely. Strong beef-tea was swallowed at intervals by our patient, but nothing seemed to revive her failing powers. The pulse never returned, and the same corpse-like coldness of the surface continued. Quart bottles filled with hot water, and covered with flannel, were placed in the axillæ, and along both sides, and under the knees, while the hot jars were kept to the feet. More brandy and beef-tea were thrown up the rectum, and were taken freely by the mouth, the stomach retaining everything. Sighing and tossing about began, and signs of sinking became manifest. It was now 1 o'clock in the day. Six hours had elapsed since her delivery, and no appearance of amendment had yet manifested itself. I felt convinced that she must die unless her downward course was checked, and I saw no other chance for her but transfusion. I knew Dr. Robert M'Donnell (who fortunately lived close at hand) had the apparatus for transfusion, and was skilled in the operation, and I summoned him and Mr. W. Colles, and Dr. Denham to a consultation. We met at 2 o'clock, and I laid my views before them. They all saw the urgency of the case, and the necessity for the operation. But just then a slight flickering of pulse was felt at the wrist, and the carotid artery gave a little stronger impulse to the finger, and it was judged right to wait some time longer. The pouring in of nourishment and stimulants was continued, and the external warmth was continued. Up to 6 o'clock matters remained in the same state, but after that every bad symptom became manifest. She became insensible; the gasping and sighing were continual, and the restlessness and tossing were constant; the countenance became ghastly, and the jaw dropped. All trace of pulse had long vanished, and death seemed very near at hand. Finding matters in this state, I determined not to let her die without making the only effort that, in my mind, could save her, and I again summoned the same gentlemen to her aid. Mr. Colles and Dr. R. M'Donnell arrived at 9 o'clock, but Dr. Denham was not able to attend.

It was now too manifest that she had not many minutes to live,

and we proceeded to perform transfusion. Her husband readily offered to furnish the blood, and about ten ounces were drawn from his arm by Dr. R. M'Donnell. This, as it flowed into a common bowl, was agitated briskly with a glass rod, in order to separate the fibrine; and the agitation was kept up after the full quantity had been drawn, and for some minutes after, until a large coagulum of fibrine adhered to the glass rod. During all this time the bowl was kept floating in a basin of hot water. The blood was then strained through a muslin cloth, and all the fibrine was thus separated. All this was done in the parlour, and the blood thus prepared was carried up to the patient's bedroom. The difficulty of the operation now commenced. The patient was as a corpse, bloodless; there was no trace of a vein to be found in her arm. A ligature tied round above the elbow showed nothing. By feeling cautiously, what we imagined to be a vein was perceived. Dr M'Donnell, who performed this operation with that skill, coolness, and dexterity for which he is remarkable, now adopted the expedient of pinching up a fold of skin at the bend of the arm, and then running a narrow bistouri through it, split it up, leaving a wide gaping wound in the skin, which displayed the veins at the bottom, resembling small flat dead earth-worms. The median basilic appeared to be the largest, and under this he passed a long acupressure needle, so as to elevate the vein, and prevent it escaping during the remaining steps of the operation. The difficulty now was to open this small, flat, empty vein without transfixing it. This he most dexterously accomplished by means of a fine pointed pair of forceps, with which he seized the anterior coat of the vein, and then with a fine tenotomy knife, the back of which was kept in contact with the vein, he succeeded in inserting its point into the vein, and slit it up above the point at which it was crossed by the subjacent needle.

The bowl of blood (which, during this necessarily tedious proceeding, had been kept floating in hot water) was now brought to the bed-side. The injecting apparatus was of the simplest construction. A common brass enema syringe that holds about two ounces such as is used for self-administration, was fitted with a fine india-rubber tube coming off from the side. This was about a foot long, and terminated in a fine glass tube four inches long running to a fine point at its extremity.[a] I held the bowl containing the blood

[a] The glass tube was used in order to facilitate the detection of any bubble of air, if such should gain entrance into the apparatus.

with my left hand, and with the right I held the brass syringe standing upright in the blood, and keeping the lower end of it well pressed to the bottom so as to prevent the possibility of any air gaining access to the interior. Mr. Colles worked the piston slowly and cautiously. The instrument was filled with blood till it ran out of the nozzle, and Dr. M'Donnell proceeded to introduce the glass tube into the vein; but this was very difficult to effect, for although a very sufficient opening had been made into the vein, it was so flat and collapsed that some time elapsed before he could accomplish his object. At length he succeeded, and Mr. Colles urged the blood forward by lowering the piston. About six or seven ounces of blood were thus poured into the system of the patient. It was intensely interesting to watch the effect produced by the introduction of this vital fluid. The first change I noticed was the improvement in respiration, the long laboured, gasping, sighing effort that had been so distressing to witness became more calm and like what natural respiration should be. This began to appear when about half the quantity of blood had been injected. The countenance became less ghastly, and imminent death seemed warded off when the last portion of blood had entered her vein. The restlessness and tossing still continuing, I gave her thirty drops of Batley's solution, and repeated the dose in half an hour; a third dose had the effect of producing sound sleep, from which in six hours she awoke warm and conscious, with a distinct though very fast pulse. She asked for food, and ate a good breakfast of tea and toast. 23rd, 12 o'clock.—Has had beef-tea and brandy twice; says she is quite well. Her countenance was bright and cheerful; pulse 140, small. She now complained of soreness of both hips, and on examining them, both were found to be burned by the hot bottles so freely used on the previous day. Two of these had escaped from their woollen covers, and during the tossing and insensible period had come into contact with the skin covering the hips. The right hip was most extensively burned, the slough which was subsequently detached from it left a deep ulcer, eight and a half inches in length by five and a quarter in breadth.[a] The burn on the left hip was about quarter the size. This will give a good idea of the condition of insensibility in which this patient was during the latter part of the previous day and night, up to the time of the operation. The low state of vitality to which she was reduced, contributed no doubt

[a] As I write now on the 8th of April, a large ulcer still exists on the right hip.

to intensify the effect of the hot bottles, for it is hard to imagine that water even at the temperature of 212° could produce so very deep an eschar as was the result of this burn.

10 o'clock, p.m.—She has slept all day from time to time, and has taken plenty of nourishment. I left a draught of muriate of morphia to be given if necessary.

24th, 8 o'clock, a.m.—She slept all night without any narcotic; pulse 120; looks bright, and makes no complaint, but of the hips; the arm gives no annoyance. Six p.m.—Has had beef-tea and toast freely all day; the bowels acted well, and slight lochia are present.

25th, 8 o'clock, a.m.—Had a very good night; pulse 115; ate a hearty breakfast; bowels acted well; voice strong; countenance good; no complaint of the arm, which healed up most kindly in a few days.

26th.—Another excellent night; pulse 100; ate half a roast chicken for dinner, and drank a glass of sherry.

From this time she progressed steadily, and after being out several days to drive in a carriage, she left town for the country on the 6th of April.

A most important point in the history of this case is the simplicity of the apparatus with which it was performed. A few shillings would cover the cost of the entire. Like many other highly important operations it is all in the using, not the costliness of the tools. It serves to show that there is no such difficulty in safely performing transfusion as we have been led to imagine, but that when the principles which should govern the operator are fully comprehended, it rests upon his dexterity and skill to carry them out, and not upon a complicated and unwieldy piece of machinery. Dr. M'Donnell accomplished, with his simple apparatus, what others possessed of elaborate machines have failed in achieving. This example of what may be done by good brains and skilful hands will, I hope, induce others to undertake the operation of transfusion, but not without a full knowledge of the essential points to be looked to, and avoided. In reading the history of the operation just detailed these points will all appear, and I will not prolong the communication by their repetition.

ART. XV.—*On the Hydrate of Chloral as a Hypnotic and Nervous Sedative, especially in Gynecological Practice.* By THOMAS MORE MADDEN, M.R.I.A.; Licentiate of the King and Queen's College of Physicians, Ireland; Member of the Royal College of Surgeons of England; Assistant Physician to the Rotunda or Dublin Lying-in Hospital; Corresponding Member of the Gynecological Society of Boston, &c., &c.

HAVING had an opportunity of testing the effects of hydrate of chloral, the new hypnotic and sedative, in a great number of cases, I venture to hope that the following observations, being a *résumé* of my notes of its action in several cases, may be of interest to those who have not employed this new remedy on an extensive scale.

Before entering on the consideration of the therapeutic value of chloral, as exemplified by the cases in which, with the approval of Dr. Johnston, Master of the Rotunda Hospital, I have administered this remedy in the gynecological wards of the lying-in hospital, as well as in private practice, I may premise that having no predisposition in favour of or against the new drug, I have given it a fair trial, and now submit the notes taken as to its effects in some of the cases in which it was administered, whether they be in favour of or against the supposed value of this addition to our therapeutic resources.

In many instances, those who have taken an active part in introducing new remedies into practice appear to have fallen into the not uncommon error of taking the *post hoc* for the *propter hoc;* and the experience of any one conversant with periodical medical literature, for even a few years past, will find highly educated and accomplished physicians extolling the virtues of novel medicines, which, within a very short time, have fallen into well-merited dissuetude. The confidence of the physician who administers a new drug of supposed extraordinary powers in the novel remedy is oftentimes reflected by the patient, who, being perhaps minutely questioned as to the effects which it was hoped might be produced by the medicine, begins to imagine what he wishes may be truly effected by the curing pill or potion; and, such is the wondrous power of the imagination over bodily sensation, that he oftentimes feels for the moment all the benefits which have been ascribed to the medicine, and unconsciously deceives his physician as well as

himself Anxious to avoid this error, I have endeavoured to test the new remedy, so as to exclude, if possible, this source of fallacy. I have, therefore, in a certain number of cases, first given the narcotics and sedatives usually employed, and have then compared their effects with those of hydrate of chloral, without letting either the patient or nurse know of the change of medicine, and I have then given some inert substance in draughts similar in form and taste to those previously prescribed, and had them administered with all the formalities with which the patient had been familiarized when taking the chloral for the purpose of the experiment; and I have thus been able to say how far the phenomena that followed were really produced by the medicine administered or not.

The great number of alleged new remedies which now-a-days are almost daily added to our list of the Materia Medica, already as long, and, in some respects, nearly as useless as the catalogue of the ships in the Iliad, should act as a preventive to our resorting to medicines simply because they are novel, unless they expressly accomplish new purposes or fulfil more advantageously those already attained; for, the unnecessary multiplication of medicines and preparations, far from adding to the resources of the practitioner, becomes, as Sir Henry Holland justly remarks, "an incumbrance to the practitioner and an impediment to the progress of the science."

Chloral, however, does promise to accomplish new purposes in the hands of the physician, as well as those of the surgeon, and to fulfil more advantageously others already attained.

The effects claimed for chloral by Liebreich, Richardson, and others, are:—1. That it produces sleep. 2. That it brings down the animal temperature. 3. That it removes sensibility. 4. Causes muscular relaxation. 5. Acts with extreme rapidity. 6. Does not require an increased dose to produce its effects, even after long use. 7. Has no counter-indication to its employment. 8. Is not followed by any unpleasant after-effects.

According to Liebreich, the physiological action of chloral is entirely due to the decomposition of the substance in the system by the free alkali contained in the blood, and the gradual formation of chloroform. This theory of the *modus operandi* of chloral has been controverted by M. Demarquay, of Paris, but has been in great measure confirmed by the more recent experiments of MM. Personne and Roussin,[a] who have shown that

[a] MM. Personne and Roussin, L'Union Medicale, Nov. 30th et Dec. 2nd, 1869.

chloral is, in the first instance, decomposed into formic acid and chloroform in the blood.

Chloral seldom produces the preliminary stage of excitement so constantly observed when chloroform is introduced into the system by inhalation; and the fact is thus accounted for by Dr. Liebreich:—"On account of there not being a sufficient amount of alkali in the blood, the formation of chloroform cannot take place, as it were, explosively; on the contrary, each molecule will take up a quantity of alkali from its immediate neighbourhood; and this process can only be finished after the blood has furnished the corresponding amount of alkali. A minimal quantity of chloroform is therefore formed in a unit of time, and acts at first on the ganglia of the cerebral hemispheres, afterwards on the ganglia of the cord, and at last on those of the heart. It is therefore, as it were, the very slowest method of administering chloroform."[a]

I now purpose to lay before the reader the observations I have made as to the physiological and therapeutic action of hydrate of chloral in the cases in which I have administered it.

Chloral is unquestionably a most powerful nervous sedative and hypnotic, but I have not found it quite as certain in its action as I had hoped from reading Liebreich's, Sir James Simpson's, and Dr. Richardson's papers; on the contrary, in some of the cases in which I tried chloral it failed in ordinary doses to produce any effect whatever. The average dose for an adult as an anodyne is from 20 to 40 grs.; usually 30 grs. acts as a full anodyne, but I have found 10 sufficient, and I have given as much as 80 grs. I would not, however, recommend the latter quantity to be rashly tried, as chloral is no mere placebo which, if it does no good, at least can do no harm; on the contrary, it is a substance which requires to be administered with great caution, being capable of acting with extraordinary power on the ganglionic and nervous system generally. Dr. Richardson states that 2 grs. of hydrate of chloral are equivalent in physiological value to 4 grs. of chloroform; and when we bear this in mind we cannot be surprised at the cases recorded by Dr. Noir in the *Gazette des Hôpitaux*, December 22nd, 1869, in which 75 grs. of chloral produced symptoms of a very grave and alarming character.

[a] Dr. Oscar Liebreich, "On Hydrate of Chloral, a new Hypnotic and Anesthetic." Berlin: 1869. See also Dr. Dobell's Report on the Progress of Practical Medicine, p. 227. London: 1870.

The case in which I gave 80 grs. of chloral, as will be seen when I come to refer to it presently (see Case VII), was of a very exceptional nature, and one in which I had very cautiously given the medicine in gradually increasing quantities from a very small to a very large dose. I have frequently found 10 grs. of chloral quite sufficient to cause the almost immediate cessation of violent uterine pain, and sometimes even to produce some three or four hours' sound sleep in cases which had resisted full opiates.

One of the great advantages which chloral possesses over every other hypnotic and nervous sedative is the rapidity of its action. In all the cases in which I have tried chloral, if it had any effect whatever, it was manifested within an hour of its administration; this was the furthest limit, but in the great majority of cases it acted within twenty minutes, and in several cases I have seen sleep produced or pain completely relieved in less than five minutes from the time the dose of chloral was taken.

It has been remarked by some observers that this drug differs from opium and other narcotics in the fact that the same dose continues to produce the same effect for long periods of constant use. This has not been my experience; for in several cases in which I continued the chloral for some time I found it necessary to increase the dose, and that after some time that even large doses failed in their effect.

Dr. Richardson states that the administration of chloral reduces the animal temperature in ordinary narcotism at the minimum 3°, and at the maximum 4°; any decrease of temperature below the latter figure being, according to this observer, an indication that the chloral has been pushed to a dangerous extent.[a] My experiments, as far as they have gone, have not confirmed this observation. In some cases I have noted a diminution of temperature to the extent of from $\frac{1}{5}$ to $\frac{1}{3}$ of a degree produced by the chloral; but in quite as many cases the medicine had no effect whatever on the patient's temperature. I have not, however, pursued my inquiry on this point sufficiently far to enable me to speak with any certainty on it.

The muscular relaxation which, as Dr. Richardson points out, is produced by chloral, indicates the probable value which the surgeon will doubtless derive from this agent in cases in which it is necessary to overcome muscular spasmodic action; and to the midwifery practitioner Dr. Richardson's hint should be of even

[a] Medical Times and Gazette, Nov. 6th, 1869, p. 567.

greater value than to the surgeon; for, as Sir James Simpson has shown, parturition has come on without any diminution of the parturient force in a patient under the influence of chloral. We have here the hint of a remedy probably of great power afforded us in the treatment of a class of cases in which I believe chloral was never thought of until I tried its effects in the wards of the lying-in hospital—I mean in cases of difficult labour from rigidity of the os uteri and soft parts. I shall now, without further preface, proceed to give the details of some cases in which I employed chloral

Case I.—*Cancer of the Bladder.*—February 6th, 1870.—K. C., aged twenty, an unmarried girl, who had been admitted to No. 10 ward August 5th, 1869. She had previously been under treatment in Dr. Stevens' Hospital; and when admitted into our chronic ward was suffering from acute pain in the vesical region and incontinence of urine, being unable to retain her urine for more than a few minutes together. The urine was very acid and turbid, containing muco-purulent matter and epithelium; her tongue was exceeding foul; her appetite very poor; her aspect cachetic, not hysterical; and her sleep broken by the incessant pain and necessity for frequent micturition. She was treated as the symptoms rendered necessary, and by various plans in succession, but with the same unsatisfactory result in all—namely, that under each she would generally appear to improve for a short time, and then as certainly relapse again. She continued in this condition for six months, when, the full opiates which she was now obliged to take twice a day to allay the pain, failed in its effects, it was resolved to try chloral, for the first time in this hospital; and accordingly on the 6th of February she was ordered—

℞.—Hydratis chlorali, gr. xx.
Mel. despumat, ʒi.
Aquæ cinnamoni, ʒi.
M.—Fiat haustus statim sumendus

Ten minutes before getting this draught she passed water. Just before it was administered at 11·30 a.m, she had intense pain in the bladder, as usual; her pulse was 110, but rather irregular, temperature normal, and pupils natural; she had also a severe headache, which she had complained of for several days. Three minutes after taking the draught she had no pain whatever; ten minutes after—at 11·40 a.m.—her pulse was 104. At 11·50 her pulse was 100, pupils slightly contracted; at 12 noon pulse 96, is

dozing; 12·30, pulse 96, sleeping; 12·40 p.m., woke up for a minute, got up and passed a small quantity of urine. She states that the micturition was not as painful as usual; the time she retained her water was 80 minutes, by far the longest period she had done so since she came into the hospital. At 1 p.m. still asleep; pulse 96. She slept till 2 o'clock, when she awoke, free from pain and no headache. She says she has not had such an interval of complete ease since her admission. At 3 p.m. the pain and frequent micturition returned, though the pain is less severe than usual. At 8·50 p.m., has some pain, no headache; pulse 92; temperature 100½, second dose of 20 grs. of chloral given. In ten minutes the pain had ceased, and her pulse had fallen to 88. She did not sleep this night, though she had no pain whatever.

February 7th.—She had 20 grs. of chloral at the morning visit, and slept for three hours after it. At 5 p.m., being in a state of great suffering, rolling on the ground with intense pain in the bladder, 30 grs. of chloral in a draught was given. This, however, only gave her a few minutes' ease, for at 6 p.m. I found her again in great pain, and gave her 35 grs. of chloral.

February 8th.—She tells me the last draught gave her more ease than anything she had yet taken; a few minutes after it she fell asleep, and slept between four and five hours, but awoke in great pain again. At 10·55 a.m. she got 30 grs. of chloral; immediately before taking it was in great pain; countenance pale and anxious; pupils contracted; tongue furred and aphthous; skin cold and clammy; pulse about 88, irregular, weak, and fluttering. Immediately afterwards her pulse became a little stronger and more regular, and the pain appeared to cease instantly. Twenty minutes after is falling asleep; complains of cold; is free from pain; pulse 84. She slept till 2 p.m., awoke crying and nervous; not in pain; appeared stupified by medicine; has not eaten nor taken her wine to-day; has only had to pass water since 11 a.m. At 8 p.m. I was sent for, and finding her again crying with the intense pain in the bladder, gave another draught with 36 grs. of chloral, which, in four minutes, was followed by complete cessation of pain in the bladder, though she still complained much of what she describes as "a queer boiling sensation or pain in the head," over the frontal region. At midnight it was again necessary to repeat the chloral, after which she passed a good night, and was free from pain next morning. The chloral was not continued in this case.

Case II.—*Odontalgia—Insomnia.*—February 27th.—E. M'D., aged thirty, a woman two days previously confined of her sixth child; was in great pain from violent toothache, which prevented her from sleeping since her confinement. She was in a semi-febrile condition from the pain; her pulse about 100; I gave her 15 grs. of chloral in a draught; this gave her immediate ease, and within a few minutes produced sleep, which lasted for three hours. She awoke talking incoherently for a few moments, but continued free from pain.

Case III.—*Puerperal Mania.*—February 6th, 1870.—An unmarried primipara, who had been delivered five days previously after a natural labour, suddenly became maniacal. As she was being dressed, to lie outside the bedclothes, as is usual, she began to shriek and clap her hands together in a hysterical manner; and after a few moments of violent excitement, became taciturn and refused to speak a word. I was sent for at once, and ordered a draught containing 20 grs. of chloral, which produced no apparent effect. She also got wine and beef-tea. Next day at noon she had an epileptic fit, and fifteen minutes afterwards, finding her very restless, excited, and loquacious, I gave her another draught with 20 grs. of chloral. Her pulse was then 100, and she complained of severe headache. After taking this draught she immediately fell asleep and remained so for four hours, when she awoke in a stupified condition; eyes wild and staring; would not speak. She had wine and beef-tea at short intervals.

February 8th.—Her condition is nowise improved. She continues obstinately silent; pulse small and very rapid; decubitus dorsal; features pinched, and her expression fatuous. Twenty grains of chloral given at the morning visit produced no effect, and at night she had 30 grs. of chloral in an enema, with brandy and beef-tea, but only slept for one hour after getting this.

February 9th.—No change in condition or treatment. She slept that night till 4 a.m.

February 10th.—She has passed from a state of melancholia and taciturnity into one of maniacal excitement and extreme loquacity. At the morning visit I gave her (as I was visiting for Dr. Johnston) gr. xxx. of chloral, which at once subdued the excitement and gave her four hours' sleep, and at night I repeated this dose with the effect of giving her, for the first time since her illness, a whole night of undisturbed repose. Next day she

appeared to be semi-stupified, as if by the medicine; was very depressed and quiet in manner, but refused to speak a word or to take any nourishment or medicine by the mouth, and that day was sent to the Richmond Lunatic Asylum.

Case IV.—*Insomnia—Threatened Puerperal Mania.*—March 30th.—Ward No. 12.—A woman, aged about forty, unmarried, was delivered of her second child after a natural labour at 10 a.m. She was in great distress of mind; did not sleep for three nights; talked wildly; got out of bed and insisted on going home the day after her confinement; manifested an aversion to the child. On the evening of April 21st she got 20 grs. of chloral, and half an hour after fell asleep, and slept for the first night since her admission into hospital, three days before delivery. Next morning she was much better.

Case V.—*Puerperal Mania*—February 16th.—A. S., aged thirty-five—ward ; incipient puerperal mania and sleeplessness four days after delivery; was given 30 grs. of chloral at 10 a.m., slept for three hours. At 8 p.m. she had a similar dose of chloral. During the night of the 18th she had an acute attack of puerperal mania, and attempted to kill her child. This was at first treated by bromide of potassium; but this failing, on the evening of the 20th she got 30 grs. of chloral; an hour after which she fell asleep, and had nearly four hours' rest. On the 21st the puerperal mania had subsided.

Case VI.—*Puerperal Mania.*—March 5th.—I was called, in consultation with Dr. O'Neil, of Earl-street, to see a lady, aged twenty-one, who had been, fourteen days previously, confined of her first child, and had afterwards progressed favourably till the tenth day after delivery, when she was suddenly attacked by acute puerperal mania of a very violent type. When I saw her she had not slept for six nights; was highly delirious; pulse 140, and weak, as she refused to take food. Two drachms of hydrate of chloral, with the same quantity of bromide of potassi, and ʒiv. of tinct. of hyosyamus in a six oz. mixture, an ounce of which was to be given every hour, was prescribed. Next day we found her worse; she had spit out the medicine, and had not slept nor took food since. We now gave her enemata of brandy, beef-tea, and 30 grs. of chloral in each, every third hour.

The third enema produced immediate effect, and she had uninterrupted sleep after taking it. The mania, however, still continued, and we found her no better the following day. The chloral was continued till the 9th, when, finding the mania unabated, though she now slept well each night, we agreed to discontinue it, and the following day she was sent to a lunatic asylum.

Case VII.—*Hysterical Mania.*—March 2nd, 1870.—Ward 11, bed 123.—A patient, aged about eighteen, who had been in the hospital for some days suffering from hysteria, was attacked by violent hysterical mania, which came on quite suddenly this evening about seven o'clock. An hour after the attack I was sent for, and on visiting the ward I found her in such a state of violent hysterical excitement that at the urgent request of the ward nurse and patients I removed her to another ward. Having before given her chloral in smaller doses, I now prescribed:—

℞.—Hydratis chlorali, grs. lxxv.
Syrupi, ʒii.
Aquæ ad., ʒii.
M.—Fiat haustus statim sumend.

8·25 p.m.—Chloral given. She declared that nothing should induce her to go to bed. With a view of keeping herself awake she paced up and down, and running on meanwhile, as the note made by the pupil who administered the medicine (Mr. E. B. Roche) states, "against the hospital and things in general." After about ten minutes it was noticed that she was walking unsteadily, and in the next few minutes she evidently could not "toe a line." She then began actually to stagger; and as the situation appeared to be suggestive, began to talk of another occasion when she was drunk. She then got into the "maudlin stage," deplored her folly, asked forgiveness; and by taking proper advantage of this state of mind, was induced to return to the much-abused bed. She soon became calm, and slept from 10·30 p.m. till 4 a.m., when she awoke quietly.

March 3rd, 10 a.m.—She wished to go home; and on being advised to remain in the hospital, became again very much excited with hysterical crying, and was ordered 80 grs. of chloral, which was given at mid-day. Her pulse was then 100. At her urgent request her mother was sent for to fetch her home, and on her arrival she insisted on going. She walked about the ward, said the draught should not set her to sleep, and got into a state

of extreme nervous excitement about going home. Gradually, however, she began to reel and stagger in her gait, and protesting that she would not lie down, easily allowed me to put her to bed. Ten minutes after the chloral her pulse had fallen to 88; her pupils were widely dilated; talking in a thick incoherent manner, but much less excited. 12·35 p.m.—Still awake, but very sleepy; can hardly keep her eyes open, though she is trying hard to keep herself awake. 12·40.—Fell asleep, and slept till 4·15 p.m.; very calm, and awoke much more composed. 5 p.m.—She insisted on her mother taking her home; is rational, but very sleepy, and staggers so much that she cannot walk across the ward unaided; appears like a patient suffering from progressive loco-motor ataxy, having no power of co-ordinating her movements. She went home that evening. Some days afterwards she again came under my care, suffering from well-marked hysteria.

Case VIII.—*Delirium Tremens.*—February 10th.—I was sent for to see a young man who had been drinking hard for several days, but had stopped drinking three days ago and did not sleep since. He was in a state of great nervous excitement; his face pale and anxious; skin cool and moist; pulse quick, small, and compressible; tongue furred and tremulous. His wife informed me that she, her servant, and two friends had passed the last couple of nights keeping him in bed, from which he was endeavouring to escape. He had taken no nourishment whatever for three days; complains of great thirst, which he has been striving to slake by copious draughts of cold water, vomited as soon as swallowed. Ordered milk and soda water in small quantities; and the following draught:—

℞.—Hydratis chlorali, grs. xxx.
Potassi bromidi, grs. xxx.
Aquæ camphoræ ad., ℥ii.
M.—Fiat haust.

This was given at 11 p.m., when his pulse was 116. In 15 minutes his pulse continued as before; in 30 minutes pulse 102—fell asleep. He slept all night; awoke more tranquil, and took some soup. Next night he had 20 grs. of chloral; slept well, and rapidly convalesced.

Case IX.—*Insomnia—Tonsillitis.*—February 13th, 1870.—A. N., aged twenty-eight—ward 10—was suffering from anteflexion of the uterus, complicated by acute tonsillitis. Not having slept for

three nights, was ordered 30 grs. of chloral, with a little syrup in three drachms of cinnamon water, as she could not swallow a larger draught. This was administered at 8·15 p.m.; her pulse being then one hundred; twenty minutes after there was no change in either pulse or temperature. She had become very excited—almost as much so as if in the excitement stage of anesthesia by chloroform. She had to be held in bed; talked loudly and incoherently; passed a sleepless night, and next morning complained of headache.

Case X.—*Acute Pelvic Cellulitis.*—February 23rd.—M. D., aged twenty-four—ward No. 10—a patient suffering from acute pelvic cellulitis. Being kept awake for some nights by the pain, was ordered 30 grs. of chloral at bed-time; this produced no effect, and she passed a sleepless night.

Case XI.—*Cauliflower Cancer from Os Uteri.*—February 16th.—J. K., aged forty-three, suffering from a large cancerous cauliflower growth from the cervix uteri, which gives rise to intense uterine pain, controlled by opiates. Every night was given 30 grs. of chloral; at mid-day, when in an exacerbation of pain, this produced immediate relief, and she had three hours' sleep afterwards.

Case XII.—*Irritable Bladder.*—February 23rd.—K. Q., unmarried, aged twenty-eight, a patient suffering from incontinency of urine; is unable to retain her water for more than ten minutes continuously. She hardly sleeps at all, being so frequently obliged to rise to pass water. There is considerable pain and soreness in the urethra and wall of the bladder. The quantity of urine voided at one time is, of course, exceedingly small. She had been previously for some time under treatment, without appearing to derive benefit. With a view of lessening the irritability of the urinary mucous membrane, and diminishing the spasmodic action of the bladder, I determined to try the effects of repeated small doses of chloral, and order a mixture containing 10 grs. to the dose, to be taken every hour. This was of decided use. The irritability of the bladder was greatly lessened. She could keep her water for two or three hours continuously, and slept soundly at night whilst taking the chloral. When it was left off all the symptoms again returned, and were not relieved until we again had recourse to it. There has not, however, I fear, any permanent improvement been effected by the chloral in this case.

Case XIII.—*Incontinence of Urine.*—February 27th.—M. J., aged twenty-eight, married; has three children; was last delivered in Glasgow three years ago, after a hard labour, and for the last two years has been suffering from incontinence of urine. This is especially troublesome at night; each morning she finds her bed saturated with urine; and, as she told me, her life had become a perfect burden to her in consequence of this infirmity. She complains much of bearing down pain in the back and hips, and also in the vagina. On examination there was no uterine displacement found, and the bladder and urethra were apparently in their normal condition. She was put on 10 gr. doses of chloral, given every third hour, with the effect of completely relieving the incontinence of urine, and allowing her to retain her water all night as long as the medicine was continued, but no longer.

Case XIV.—*Irritable Bladder.*—April 10th.—Ward No. 10.—A girl suffering from an irritable bladder, with acute pain in the vesical region, was ordered five grains of chloral at 10·35. The patient was then suffering a good deal of pain. Pulse 104.

At 11 o'clock the medicine was administered. In twenty-five minutes the pain was found to be somewhat lessened; the pulse was 96; and although the girl did not lie down she showed unmistakable signs of sleepiness.

Another dose was given, which completely removed the pain, causing decided drowsiness, with some headache.

Case XV.—*Hysterical Peritonitis.*—K. S., aged eighteen, married, was admitted to 10 ward, February 23rd, 1870. Four months ago she received a kick in the abdomen, and since then she has never been free from abdominal pain. This has been most acute for the last three weeks, and has confined her to bed. For the last three months her appetite has been very bad, and she acknowledges that she has lived chiefly on brandy and whiskey during this time. When she came to the dispensary she appeared half mad with the abdominal pain; could not stand nor answer my questions, and she repeatedly assured me that she had not slept for the last three weeks. The abdomen was small and contracted, the muscles tense and rigid, the lips drawn up, and such extreme tenderness as to render an examination difficult. She was ordered fomentations, poultices, and a draught with 35 grs. of chloral at once, to be followed by a castor oil draught with

tincture of hyosyamus in four hours. At 11·25 a.m. she got the chloral, her pulse being then 68, soft and compressible; temperature in axilla 99·8; six minutes after taking draught feels sleepy; abdominal pain still continues; pulse and temperature unchanged. At 11·55 a.m. she fell asleep, and slept soundly till 4·40 p.m. Her pulse was then 68. Soon after waking she became very excited; talked incoherently; got sick, and retched a good deal. At 9·40 p.m., as she was again complaining loudly of the extreme abdominal pain and pain in breast, with great tenderness, especially in the supra-pubic region, her pulse 72, temperature 100, pupils normal, tongue clear, but very red, she got a draught with 45 grs. of chloral in 2 oz. of cinnamon water. She objected thoroughly to the acid taste of the medicine. Two minutes after taking it pain in abdomen greatly relieved; twenty minutes afterwards pulse 78, and softer; temperature had fallen 0·5; pupils widely dilated; is very flushed, excited, and talkative. She then soon fell asleep, and slept till 3½ a.m., when she awoke complaining of the pain as before.

February 24th.—She was still complaining of great pain at the morning visit. She was ordered a turpentine draught with acetum opii; and in the evening, as the pain and restlessness continued unabated, I ordered 55 grs. of chloral in a draught. By some mistake she did not take this. I found next morning that she had contrived to smuggle in some whiskey, after taking which she slept as well as after the medicine.

25th.—Her condition was much the same as the previous days, and she was now ordered 65 grs. of chloral in ʒii. of peppermint water. She got sick immediately after swallowing this; however, she fell asleep half an hour afterwards. At 8 p m. she was in great pain and begged loudly for an anodyne. I resolved to try the effect of a larger dose of chloral than I had yet administered, as I gradually increased the dose day by day up to the present. I prescribed—

℞.—Hydratis chlorali, grs. lxx.
Syrupi, ʒii.
Aquæ cinnamoni, ʒi.
M.—Fiat haust.

Immediately before taking this, appeared in much pain, and manifested extreme tenderness over uterus. Pulse 116; temperature normal. Five minutes after taking the chloral began to talk in an excited and incoherent manner; begged me to cut her open

and see what was wrong with her. Ten minutes after she fell sound asleep. Pulse 112; respiration slightly stertorous. Thirteen minutes after medicine, pulse 100. At ten o'clock I found her fast asleep; respiration perfectly natural; pulse 80. At midnight she awoke, went over to the fire, returned to bed, fell asleep immediately, and remained so till past 6 a.m.

February 27th.—She is complaining just as before; and, in order to try whether the relief she stated she had derived from the chloral was really the effect of the medicine or not, with Dr. Johnston's approval, after the same formalities of taking her pulse, temperature, &c., with which she was familiarized when getting the chloral, a draught of sweetened peppermint water was administered.

This, however, produced no effect whatever; and at three o'clock, when I visited the ward, finding her in a state of great suffering, I resolved to try the effect of repeated small doses of chloral, and accordingly directed:—

℞.—Chlorali hydratis, ʒi.
Syrupi, ʒi.
Aquæ menthæ pip., ʒv.
M.—Fiat mistura cujus sumat, ʒi. quaqua hora.

until the pain ceases or she sleeps. The first dose produced sleep; and each time the pain returned it was repeated with the same result.

Case XVI.—*Hysterical Peritonitis.*—M. C., aged twenty-five, a patient in No. 11 ward, suffering from hysteria. At 11 a.m., April 20th, was apparently in a good deal of pain, referred to the abdomen. At 11·5 a.m. hydrate of chloral, grs. xxxv., in cinnamon water was given; five minutes after eye-lids drooped, appeared drowsy, and suffering less pain At 11·30 effects of chloral seemed wearing off, as shown by restlessness and moaning. Pulse 116; temperature 103·8 (at 10·30 pulse 124; temperature 103·1). At 11·45 fell asleep, and continued so until 12·10 p.m.

Case XVII.—*Rigidity of the Os Uteri during Labour.*—February 28th.—A primipara, aged twenty-two; ward 2; admitted in labour; has been in labour since 10 p.m. on the 26th. On the 27th, at 11 p.m., os size of florin, thick and rigid. On the 28th, at 8·45 p.m., the os being still in the same condition, though in the meantime she had been treated in the ordinary manner, by warm baths, opiates, and solution of tartar-emetic, I resolved to try the effect of chloral, and ordered the following draught, which was given at 8·50 p.m., her pulse being then 100:—

℞.—Hydratis chlorali, grs. xl.
Mel despumat, ʒi.
Aquæ cinnamoni, ʒi.
M.—Fiat haust. statim. sumend.

At 9 p.m.—Asleep; pulse 90. 9·10 p.m.—Asleep; pulse 88. 9·30 p.m.—Asleep; pulse 88. 9·38 p.m.—Roused up for a moment, had a pain, and settled down again. 10 p.m.—Asleep, not very soundly; was examined; no change in condition of os uteri. 10·15 p.m.—Having been restless and in pain for the last quarter of an hour, was ordered a second draught with 20 grs. of chloral; in all, one drachm in one hour and twenty minutes. After taking draught she at once fell asleep again. 11 p.m.—Still sleeping soundly. At 3 a.m. she woke up, and at 4·30 a.m. she was delivered.

Case XVIII.—*Rigidity of the Os Uteri during Labour.*—March 7th.—A case of rigid os in a primipara, aged twenty-four. Strong labour had gone on for 24 hours; membranes ruptured; os thin and rigid, continuing the size of a shilling. Chloral was given in ten grain doses without any apparent effect for three hours. The warm bath was now resorted to with great benefit; she had four hours sound sleep; the os then relaxed, and delivery was easily effected with the forceps.

Case XIX.—*Labour Retarded by Rigidity of Os Uteri.*—April 9th.—I was sent for to see a lady, aged thirty, who had been for forty-eight hours in labour. It was her third confinement. On examination I found the os the size of a florin, thin and rigid; pains strong; pulse weak and rapid; patient exhausted by the protracted suffering. I ordered a warm bath, an aperient enema, some beef-tea, and a draught with 20 grs. of chloral at once. In two hours I found the os the same size and the pains much weaker. She now got another stimulating enema of salt and water, and afterwards another warm bath. She was chloroformed at her own request (as she had been another time, during her last labour in Malta), and kept under the influence of the chloroform for three hours. No advance being made I gave her a second draught with 20 grs. of chloral at 8 p.m., and this being rejected I gave a third at 9 p.m. At midnight I found the os nearly the size of a crown, but still rigid and undilatable. She had a third warm bath, which appeared to relax the rigidity more than anything else; and at 6 a.m. the os

was so far dilatable that I was enabled to deliver her with the long forceps.

Case XX.—*After-pains.*—April 16th.—M. A. L., aged twenty-six, delivered of her third child, after a natural labour, at 7 p.m.; at midnight was complaining loudly of after-pains, and by her outcry kept the other patients in the ward awake. I prescribed 15 grs. of chloral as a draught at once; three minutes after taking this the pains were relieved, but she did not sleep that night. Next evening she again complained of uterine pains, for which 20 grs. of chloral was given; this produced almost immediate sleep.

Case XXI.—*After-pains.*—April 17th.—T. N., aged twenty-two, second pregnancy, confined in the same ward as the last patient, also complaining of after-pains; had a draught with 20 grs. of chloral at 9 p.m., immediately after taking which the pains ceased and she slept all night.

Case XXII.—*Acute Ovaritis after Delivery.*—April 17th.—Puerperal patient, two days after delivery complained of great abdominal tenderness, especially in left iliac region; no tympany; no pain except on pressure; lochia natural; pulse 104; tongue moist and clean; not having slept since the 14th, was given two drachms of Long's aromatized chloral, being equivalent to 20 grs. of chloral, at 11·30 p.m. Eight minutes after taking this she was asleep and slept for three hours. Next morning the tenderness returned, and her pulse was 136. She is still under treatment.

Case XXIII.—*Insomnia.*—February 28th.—A. G., aged twenty-six, suffering from debility after confinement and hemorrhoids. A very nervous excitable woman, unmarried, in a very despondent frame of mind. On the nights of the 25th and 26th slept very poorly; last night did not sleep at all. When she doses is awakened by fearful dreams; manner abrupt and peculiar; appears not far from being insane; tongue dry and covered with white fur; pulse 104; countenance pale and anxious. Complains of great headache and palpitation.

℞.—Chlorali hydratis, ʒi.
Tincturæ valerianæ ammoniat., ʒiii.
Infusi valerianæ ad., ʒvi.
M.—ʒi. 2 da. quaqua hora.

March 1st.—After taking the second dose of the mixture she went to sleep; passed a good night, and awoke decidedly better. This medicine was then continued for some days with marked benefit.

Case XXIV.—*Insomnia.*—April 19th.—Patient 125.—Ward 11.—Had not slept for two nights. At 12·5 p.m. two drachms of the syrup of the hydrate of chloral (20 grs.) were administered in water. The patient lively and talkative, was not suffering any pain; pulse 95. Within one minute after swallowing the draught her eyelids drooped, and she said she felt drowsy. At 12·10, *i.e.*, in five minutes, she was sound asleep, breathing quietly and naturally. Awoke as from natural sleep; no headache or other unpleasant symptom.

Case XXV.—*Insomnia.*—April 19th.—Patient 124.—Ward 11.—A patient suffering from insomnia and cellulitis after delivery; in place of usual anodynes a similar draught to the above was given at midnight. Draught almost at once ejected from stomach. It, however, had time to be, at least, partially taken into the system, for this patient, whose pulse was 120, and who complained of much pain, was asleep in a few minutes, and slept until morning.

Case XXV.—*Ulceration and Sub-acute Inflammation of the Uterus.*—Mrs. B., aged twenty-eight, married ten years; has three children; the youngest of them is eight years old. For the last four months she has been under my care, suffering from ulceration of the os uteri, sub-acute inflammation of the uterus, and occasional sudden brief successions of intense pain, referred to the region of the left ovary. Before she consulted me she had been under the care of an eminent London physician, and before that had been treated by a military surgeon at Netley. I need not pursue the history of her case further than to say, that under appropriate treatment the ulceration was cured, and the other uterine symptoms subsided. But the neuralgic paroxysms of ovarian pain continued to recur every ten or twelve days, and harassed her for two or three days together. Various plans of treatment were resorted to ineffectually, and she was ultimately directed to take $\frac{1}{16}$ of a grain of perchloride of mercury in 5i. of compound tincture of bark three times a day, and equal parts of the unguentum opii, hydrapyri, and iodi were directed to be rubbed in over the seat of pain. Whilst pursuing this treatment she came to me on February 18th in such a paroxysm of pain that she could not stand straight, being bent like a very old woman, to relieve the pain; her pulse 120, and her countenance expressive of great suffering. I sent for some chloral, and gave her 20 grs.; this

produced almost instantaneous relief; her pulse fell to 90; her face, from being pale and anxious, became rosy and cheerful, and she was enabled to walk home. I repeated this treatment on two subsequent occasions with similar effect.

Besides the foregoing, I have given chloral in twelve other cases; but the space at my disposal does not permit me to give the details of them in the present communication. One was a case of slight *delirium tremens*, a middle-aged gentleman, cured by 10 gr. doses of chloral, repeated every third hour till sleep was obtained. One was a case of insomnia, acute gastritis, and some mental disturbance occurring in a patient ten days before her confinement, and suffering from the abuse of wine and ardent spirits, of which she consumed an almost incredible quantity. In this case, which I saw with my friend Dr. Moore, of Amiens-street, chloral, given in large doses, failed to produce sleep, which followed the substitution of bromide of potassium for the chloral. Two were cases of irritable bladder. Two cases of rigidity of the os uteri during labour, in which chloral appeared of some use. Two were cases of puerperal mania, in one of which 15 gr. doses of chloral produced sleep; in the other 35 gr. doses had no effect; and lastly, four were cases of after-pains, relieved by from 5 to 20 gr. doses of chloral.

In some of the foregoing cases chloral failed to produce sleep; in two or three cases it not only failed to produce sleep, but, on the contrary, induced a condition of extreme nervous excitement and hyperesthesia. But in the great majority of cases the chloral allayed nervous excitement, relieved pain, or produced sleep with great certainty and remarkable rapidity. I have, moreover, observed the fact that the sleep produced by chloral more closely resembles natural sleep than that induced by opium or any other narcotics; the patients were generally free from any unpleasant after-effects, such as those produced by opiates, and informed me that they had slept pleasantly, as well as soundly, and awoke refreshed by their night's repose.

In conclusion, I may venture to say, speaking from my own experience of this substance, that hydrate of chloral is one of the most valuable hypnotics and nervous sedatives at the command of the physician. And I might venture to hazard the opinion that ere very long hydrate of chloral will come to be tried in every case of insomnia, from whatever cause it may arise, and in every case of

acute nervous pain and hyperesthesia, in which opium or other narcotics and sedatives have either failed to produce sleep or allay pain, or are contra-indicated by any cause.

ART. XVI.—*Postscript to a Case of "Diffuse Hypertrophy of the Mammæ."* By S. M. MACSWINEY, M.D., Physician to Jervis-street Hospital.

IN the Journal of November, 1869, I related the medical history of a young woman, an hospital patient of mine, who was the subject of great hypertrophy of the mammæ; and for whom my colleague, Mr. Stapleton, under whose care for surgical treatment I had placed her, removed the right breast by excision, after all other means we had recourse to had totally failed to reduce the organ in size. In that communication I mentioned that removal of the breast at the time had become necessary to save the patient's life, and I explained the grounds upon which mainly was based the decision to amputate but one breast, in the first instance at least—the expectation, namely, that this proceeding might be followed by a permanent reduction in the size of the other enlarged gland, as had heretofore happened in several examples of tumors of a like nature (and where but one breast had been excised) recorded in the annals of medicine. I further related in that paper how, after the operation, the remaining organ had greatly diminished in size; and that I even entertained sanguine hopes that it would ultimately assume a normal and permanently quiescent state; but at the same time I fully recognized the possibility of a different result; and in fact, I made the following observations:—"Should this sanguine expectation, however, not be realized, as is quite possible, and should, on the contrary, the left breast—instead of returning to, and remaining of its natural size—resume the process of abnormal growth at present suspended in it, then in this event, I will not hesitate to advise *its* removal also."

At the time when I sent the report of the case to the Journal, the process of involution of the left mamma was still actively going on. This action, however, shortly after this period, quite suddenly ceased; the size of the organ remained stationary for some weeks, then it began to enlarge again, and soon reached a dimension fully equal to what it had previously attained.

During this time both Mr. Stapleton and I frequently saw this

girl, who had returned home, and some internal remedies, and external applications were prescribed for her, but they did not exercise the least influence in retarding the process of growth, which now again became very rapid. Her health, concurrently with the renewal of the action of hypertrophy, once more failed her, and ultimately she was again received, greatly broken down in strength, into hospital, on 5th January, 1870. I measured the gland on this day, and the dimensions were as follows, viz.:—circumference, 30 inches; length, 22 inches.

The surface of the organ was unhealthy-looking; dark red in colour, and very tender to the touch. At one point the skin had given way, and a sero-sanguineous fluid was being discharged in small quantity. It was plain to see that a process of low, inflammatory action was taking place in the gland, which might, unless carefully watched and treated, end in some serious disorganization. The girl's pulse was quick; her tongue was loaded with a brownish yellow coating; her appetite was bad; and she complained of feeling weak, and very sick.

Anxious as Mr. Stapleton was to afford the poor girl the great relief, which removing the enormous mass of enlarged gland which was wearing her life away would, it was almost certain, give her, he nevertheless felt himself constrained to wait until, by rest and suitable treatment, the local disturbance should be subdued and quieted, and the general health somewhat restored.

It required some considerable time to accomplish this; but at the expiration of about a month, her condition was so much improved, that it was decided not to postpone the operation any longer.

Accordingly, on 10th February, the girl was put under the influence of chloroform, and Mr. Stapleton removed the breast in an unusually rapid manner, and almost without the loss of any blood. The breast, put in the scales, was found to weigh 11 lbs.

The following notes of the case, after operation, were taken by Mr. D. M'C. Ross, my clinical clerk, who watched the patient's progress with great interest and care:—

Immediately after the operation, she got a draught containing liq. morphia hydrochl. m. xx.; acid hydrocyanici dil. m. i. 7 p.m., same day; her pulse was 94, and full; felt chilly; vomited, which relieved her; has no headache. 9·30 p.m., pulse 108, and full; did not experience another chill, nor did she vomit a second time. Has been getting chicken broth, ice, &c., and now requests a little lemon juice, as she feels thirsty.

11th February.—Pulse 128; did not sleep very well during the night, but is sleeping in the morning; no headache or vomiting; no hemorrhage; was ordered soda in effervescence with 20 min. liq. morph. hydrocl., and 1 min. acid hydrocyanici dil. in each dose, every third or fourth hour.

12th February.—Pulse 128; tongue coated with a white fur; complains of great pain all down her left side, which she says prevents her from breathing.

13th February.—Pulse 112; tongue not so much coated to-day; has still the pain in the side; Dr. Stapleton removed all the dressing except two pieces of plaster, which had drawn the edges of the wound together; there was not much discharge; it was re-dressed with lint soaked in port wine; allowed to get some chicken and wine, and to continue her sedative mixture.

14th February.—Pulse 94; tongue cleaner to-day; not much discharge, and what there was looked healthier than yesterday; re-dressed the same as on the previous day; ordered an iron mixture, but at the same time to continue the sedative effervescing one occasionally; same diet.

15th February.—Pulse 88; tongue perfectly clean; to-day the wound was dressed in the same way; a part of the incision has healed.

16th February.—Pulse 80; tongue clean; sleeps well; no change in the general symptoms; no alteration in the diet.

17th February.—Pulse 72; sleeps well; tongue clean; same treatment.

19th February.—Pulse 68; dressed again the same way; the incision is rapidly healing.

24th February.—Pulse 68; still continuing the same dressing; is healing very rapidly; there is only a small piece to unite in the centre.

28th February.—Pulse 68; her improvement latterly has been so uniform, no lengthened remarks were necessary; the incision has now completely healed; she looks in good health; her appetite is very good; and she sleeps very well.

11th April.—On this day the patient left hospital The wound was healed; the health completely restored. She had already grown very fat, and expressed the utmost gratitude for the freedom from great suffering which the operation had afforded her.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Étude Sur Les Maladies Éteintes et Les Maladies Nouvelles. Par Charles Anglada, Professeur de Pathologie Médicale a la Facultè de Montpellier, &c. Paris: J. B. Bailliére et Fils. 1869. 8vo, pp. 646.

A Treatise on Diseases, Extinct and New. By Charles Anglada, Professor of Medical Pathology to the Faculty of Montpellier.

In the absorbing pursuit of the medicine of the present day the inheritance of the past is in danger of being forgotten. We have in a great degree neglected the counsel of our illustrious Sydenham, to study thoroughly the constitutions of epidemic diseases, and consequently have advanced but little since his time towards the solution of those difficult problems. Our literature is poor in works relating to archæological medicine, and we possess no description from the pen of a physician of any of the great pestilences of the earth, which can rival the non-professional histories of the plague of Athens by Thucydides, of the plague occurring in Justinian's reign by the eloquent Gibbon, and of the Black Death by Boccacio. We have indeed no good monograph on this subject in the English language since Mead's writings on the plague, up to the first year of the present century, when the American physician, Noah Webster, published his history of Pestilential Diseases. This can still be read with interest, and forms the text-book from which all subsequent writers largely borrow. In Bascome's work on epidemic pestilences we are disappointed in finding no references given to substantiate his statements. In Germany, the "Chronik der Seuchen," by Schnurrer, appeared in 1823, and it contains, along with much of the marvellous, a large store of valuable information. Since this came out we are much indebted to the German physicians for important works on pestilential diseases, and we need only allude to the "Morborum Antiquitates" of Gruner, to the "Mediæval Epidemics" of Hecker, and to the works of Hirsch and Haeser.

In France, until of late years, this ground has been but little cultivated, and we welcome with pleasure Dr. Anglada's interesting work. The author is Professor of Pathology to the Faculty of Montpellier, and is the son of the late Dr. Joseph Anglada, who was Professor of Medicine in the same University. The father was favourably known for his researches on toxicology, and the son has published during the last thirty years various works on subjects of general medicine. A treatise of his upon contagion appeared in 1853, and elicited the commendation of the celebrated Trousseau, who in his *Clinique Médicale* has adopted as his own the definition of Dr. Anglada, "Celle (definition) de M. Anglada, de Montpellier, me paraît mieux rendre l'idée que je m'en fais." The present work of M. Anglada is one upon a subject much contested, and is modestly called by him an étude, or inquiry, into diseases which are supposed to have become extinct, and into others which appear to be new. Though we readily concede to him the merit of originality in the *title* which he has chosen, yet the subject of his researches, in part at least, has occupied the attention of such able medical historians amongst his countrymen as Ozanam, Daremberg, and especially Littré.

The table of contents of the present volume consists of an introduction of fifty pages, in which the author discusses the general relations of his subject, and of ten chapters, in which he narrates the history of those great pestilences which desolated the earth. Like the fifteen decisive battles of the world, so vividly described by Professor Creasy, which stand forth as landmarks in history to denote the changes of empires, so do those ten plagues appear conspicuous, some of them as overturning ancient dynasties; all of them as overwhelming for the time social order, and moral and religious obligation, in one stupendous cataclysm. He heads his terrible list with the famous plague which led to the ruin of Athens in the fifth century before our era. The second chapter describes the great epidemic which occurred in the reign of the Antonines, in the second century after Christ. The third epidemic took place in the third century, and was known as the plague under Gallus. The fourth was the great bubonic plague of the sixth century, which committed such ravages in the reign of Justinian. The fifth was the great epidemic of the new eruptive fevers—small-pox, measles, and scarlatina, which dated also from the sixth century. The sixth in order was the gangrenous epidemic of the middle ages, known as "Mal des Ardents," or St. Anthony's Fire. The seventh was the

Black Death, or, as it was called in Italy, la Mortalega Grande, the Great Mortality, which raged in the fourteenth century. The eighth was la Suette Anglaise, Schweissfieber, or English Sweating Sickness, which began in the fifteenth century. The ninth plague was the great Syphilitic epidemic of the same age. The tenth and last chapter is devoted to the grand pestilence of the nineteenth century, the Asiatic cholera. However painful it may be to modern civilization to confess the fact, the last of this black catalogue of ills, as if to rebuke man's overweening confidence in his power, both in the extent of its diffusion and its great fatality, stands second to none of its predecessors.

The question to which the author addresses himself in the introduction is twofold—first, whether diseases which once were known to have prevailed have ceased to exist; and, secondly, whether new diseases have, from time to time, appeared in the world? The question is not wholly novel, for we find it proposed for serious debate some twenty centuries ago by Plutarch, who, in fact, gets up a little stage-scene to give the discussion a dramatic effect. For example, a circle of inquiring citizens is assembled in the lecture-room, the subject of the evening is given out—"Can new diseases be engendered?" The disputants are, on the one side, Plutarch in person, who appears as the advocate of freedom of opinion; on the other, a physician, who opposes the question. Plutarch argues that leprosy and hydrophobia were not known before the time of Asclépiades; the physician sticks to the good old maxim that the laws of nature are unchangeable. "What," he exclaims, and his hearers applaud him, "are we to suppose that nature acts in our bodies as men would do in a town, and that she comes out according to the fashion of the day, as an inventor of novelties?" It appears, however, that the physician is beaten off the field, for Plutarch concludes that, "without going beyond our own case, changes in the habit of living form a sufficient cause for the production of new diseases."

In fact the two hypotheses proposed to explain the origin of diseases are still open to discussion. The *realistic* theory holds that pathology is invariable, that our diseases are as old as the human race itself, though dependent on modifying circumstances for their appearance; consequently they cannot be regarded ever as being either novel or obsolete; that diseases arising from our surroundings, for instance catarrhs from atmospheric changes, are admittedly contemporaneous with our existence and never leave us;

that the specific characters which diseases evince favour the doctrine that they have originally been modelled after types, which are immutable and pre-established to act in harmony, upon which types depends that uniformity of character which is the basis of our knowledge of diseases and of the descriptions of them in our medical works. It would follow from such a view that though some diseases have appeared rarely, or but once, we are not thereby hastily to conclude them to be extinct, as judging from the uniformity of nature, and her care in economizing her materials, they may reappear, when a like concatenation of causes occurs again.

The second theory is founded upon an *a posteriori* principle, and appeals to observation and experience for support. It denies that pathology is unchangeable, and maintains that all history teaches us that certain diseases, arising out of some new cause, or combination of causes, not known to us, come up at times to the surface, like bubbles on a stream, last for a brief period, and disappear from sight and existence together. It illustrates its view by pointing to the fact that genera and species of animals and plants, now extinct, are every day disentombed from the bowels of the earth; and that consequently analogy allows us to conclude that diseases may be ranked in a similar category. Further, it holds that an unbiassed examination of the records of diseases show us new and strange forms of maladies, the very date of whose appearance can in many cases be assigned, while we ourselves are at a loss to identify some of the diseases of ancient times. The author, we need scarcely say, strenuously upholds the dogma, that diseases, like their subjects, can both be born and die.

In order to lay down clearly the terms of the question, the author premises that there are two ways by which diseases are formed.

"Les maladies se forment de deux manières, par *réaction* et par *affection*. La confusion de ces modes pathogéniques a été le vice radicale de la doctrine de Broussais."

"*Reactive* diseases are those whose first phenomenon is a morbid act, which immediately follows an injurious external impression. Their symptoms vary with the exciting agents, and correspond, with some exceptions, to the nature of the sentient impression, and to the extent of injury which has been the result of it. These diseases are bound so closely to their initiating causes, that it lies in our power to produce them at will, with a reserve for vital contingencies."

On the other hand :—

"*Affective* diseases represent a general morbid state, long in preparation, and holding in dependence eventual local determinations. Their most common origin is in obscure and undiscoverable causes, and we are not able to determine their relations with the ordinary morbid influences. Vital activity must therefore be admitted to form of itself a sufficient reason for the change which is effected in the normal condition of functions and organs. This is what we express by saying that those maladies are *spontaneous*, or referrible to some unaccustomed modification of healthy life, without the apparent concurrence of an external cause."

According to the preceding distinctions, spontaniety stands in open antagonism to external causes as a source of diseases. Still the author is forced to acknowledge the great difficulty of keeping the two distinct from all mutual interaction.

"Quand nous disons qu'une maladie est spontanée, nous ne prétendons pas affirmer, pour tous les cas et d'une manière absolue, qu'aucun facteur externe n'a pris part à sa production. Mais il reste toujours vrai qu'on chercherait vainement dans les agressions extérieures la cause prochaine des maladies affectives."

Reactive diseases must be greatly affected by differences in the civilized state. Thus, the art of war is fruitful in bringing out new kinds of fire-arms, which produce new forms of mutilation of the human body. Steam, by its introduction into industrial occupations, is not slow to create injuries of its own. Poisons, hitherto unknown, come into use, acting insidiously and injuriously upon the human frame. The imperative demand for things which were deemed luxuries a little ago, but are now become necessaries of life, keeps the working classes at an unnatural strain to bring the article to market in the shortest time, and which leads them to peril life, and regard sanitary precautions as slow. Amongst a host of examples of those new varieties of reactive disease, he cites the cases of necrosis of the maxillary bones, which occurred in the makers of lucifer matches from the vapours of phosphorus. Since the white phosphorus has been replaced by the red, this new form of disease has almost become a medical souvenir of the past.

Having laid down his two modes by which diseases are formed, the author proceeds to apply them to the explanation of the great pestilences which have produced such wide-spread havoc in the

world. Finding no external cause, or chain of causes, which could bring conviction to his mind as a sufficient reason for such prodigious effects, he feels himself logically compelled to assign these effects, by the way of exclusion, to causes working within our bodies.

A great number of spontaneous affections are *specific*. They are the work of slowly-acting causes, and present examples of the highest degree of morbid individuality. He seems to regard them as developments from the modes of living usual in societies. Gout, tuberculosis, herpetism, have had different dates of appearance in the world, and the slow elaboration of time is a necessary element for their incorporation in the human system. As M. Littré has well observed, "diseases have their chronology as well as their geography."

Dr. Anglada gives examples of geographical diseases, which are entitled to the character of novelty, as being endemic in particular countries, which possess the exclusive monopoly of them. But the fact of being indigenous to one country, would seem rather to refer their origin to obscure local causes than to purely internal elaboration.

"La Plique de Pologne, le Bouton d'Alep, le Sibbens D'Écosse, la Radézyge de Norwége, la Lèpre d'Égypte, le Pian d'Amérique, le Yaws des côtes de Guinée, le Tara de Sibérie, le Waren de Westphalie, la Fégarite d'Espagne, le Mal de la Rose des Asturies, le Ginklose d'Islande, le Noma de Suède, la Chilolace d'Irlande, représentent autant d'espèces morbides qui ne trouvent les conditions de leur développement, que dans le concours indéterminé de certaines influences topographiques." "The history of voyages and travels," he continues, "is every day enlarging the sphere of this observation, and it is no exaggeration to say that certain regions have their pathology, as they have their fauna and flora."

The influence of geographical position upon the health and diseases of the human body has been long since remarked, and may yet lead to some important discoveries upon the origin of contagions. Cicero has made the suggestive observation that our manners of life, and moral feelings, are more influenced by the *natura loci* than by family and race. "Non ingenerantur hominibus mores, tam a stirpe generis ac seminis, quam ex iis rebus quæ ab ipsâ naturâ loci, et a vitæ consuetudine, suppeditantur, quibus alimur et vivimus (*De lege Agrariâ*, cap. 35). The habits and

customs of people, the climate, mode of living, tastes, must, more or less, modify and transform both the inward and outward man.

In the list just quoted, Dr. Anglada includes as a well known indigenous disease of this country, "*la chilolace d'Irlande.*" We are not aware of any disease under this name peculiar to Ireland. On referring to Ploucquet, who has described it, we find a disease called *cheilocace, seu labrisulcium*, but it is not put down by him to our account. Rayer, in his vocabulary, describes it as a *tumefaction of the lips*, occurring in children of four or five years of age. "The lips become hard, and by separation from the teeth and gums give to the physiognomy an appearance quite unnatural. Sometimes they are divided into two parts by a deep fissure, whence flows a sanious humour. The upper lip is sometimes the only one affected, when both, the upper is the most. The English call it "the mouth's canker." We do not think this ailment to be so constant with us as to deserve the name of a national disease. We find no record of it in the works of Boate, Tuomy, Rogers; or O'Connell, and even Rayer, from whom probably Dr. Anglada borrowed it, rests its pretensions as an Irish endemic upon an "on dit."

Amongst the diseases, once prevalent, but now all but extinct, except in its retreat in the East, is leprosy. This terrible disease of the middle ages has left its mark in the numerous leper hospitals which Christian piety raised for its relief. The author states that in the thirteenth century there were 2,000 of these leproseries in France, and quotes Matthew Paris as authority for the statement, that there existed 19,000 of those institutions in Europe. This prodigious number, however, appears a mistake, arising out of the circumstance, that the Knights Hospitallers, who had the care in a great degree of the leper-houses, possessed that number of manors, "Habent hospitaliarii novemdecem (millia) maneriorum in Christianitate." These hospitals were open to other skin diseases besides leprosy, and especially to the elephantiasis of the Greeks.

In the catalogue of diseases which have become extinct are enumerated the plague of Athens, the morbus cardiacus of antiquity, the black death, and the sudor Anglicus. In that of maladies, once broadly diffused as pestilences, but now "amendeés," appear the names of scorbutus, syphilis, pellagra, and others. On the other hand, if some diseases have retired into narrower limits, on a compensating principle, others have received an increase in

extension and intensity. Diphtheria, he says, was certainly known to the ancients, but it scarcely quitted its sporadic form until the present century, when it burst like a meteor on France, Holland, and a great part of Europe. A careful examination of the medical journals would give us the accounts of strange forms of disease appearing from time to time. He mentions as such the clignotte of Guipuscoa in 1849, so called because those seized with it died in the twinkling of an eye, "dans un clin d'œil." He quotes Mackenzie for an inflammatory ophthalmia consequent on a form of bilious fever, like the febris ardens of hot countries, which, he says, "regne à Dublin." We may suggest a remarkable example of these intercurrent diseases in the recent so-called cerebro-spinal fever.

The author has laid down a practical division of epidemics into little and great. The *little* he makes to depend upon defects in our physical and moral hygiène, so that he lays upon ourselves very much of the responsibility of their origin, development, and reproduction. Perhaps this is made too much of. In Ireland, the spread of typhus fever is not altogether to be imputed to our fault, but other factors must be taken into the account. Famine, for example, is as surely with us the progenitor and associate of fever, as the λοιμος and λιμος conjoined by Hesiod. The *great* epidemics, properly so called,

> "Naissent par les seules forces de la nature, c'est à dire qu'aucune combinaison humaine ne peut en préparer et en provoquer l'explosion."

He lays down a summary of the most notable marks by which we may recognize their individuality. These are, "appearance after long periods of intermission; suddenness of invasion; etiology unknown and without any appreciable relation with ordinary causes; fatality rebellious to all the efforts of art; profound specificity; aspect strange, and without anything analogous amongst known diseases." There are some diseases, such as influenza, which possess some, but want others of those marks. They are in reality common diseases gifted for a time with some accidental force of radiation, but can be referred in their source and extension to the common laws of pathology. He admits at the same time a likeness to the great epidemics in some of their less usual characters. They have, he says, in their constitution a something, he knows not what, but to which, for want of a better name, he would give the title "d'élement epidemique." He may

allow us to suggest a designation which the great Roman jurists were fond of applying to subjects in civil law, which were like the reality, but still not it. We would call them, *quasi-epidemics.*

Our author naturally asks, "d'où proviennent les grandes epidémies?" and we re-echo his question. We fear that we must be contented as yet with the "quid divinum" of Hippocrates. One of the latest hypotheses is that proposed by M. Fuster in his book on the diseases of France. This writer is of opinion that he has discovered the secret to exist "in an undetermined combination of cosmical causes, and of moral and political influences." This is, in point of fact, a revival of the doctrines of Noah Webster, who tries to prove that strange commotions in the physical world used to precede and accompany the great pestilences. M. Fuster would add great moral perturbations to these conjoined causes.

The only way to throw a ray of light upon the mysterious origin of epidemics, is to note down carefully and without bias the points in which they agree, and those in which they differ; the characters which never are absent, and those only accidentally present; the centre from whence they seem to proceed, and the barrier where they cease to spread. Also, how far their manifest preference for some places or persons, and their indifference to others, may be found to arise from caprice or from law. In all probability this is a question which we shall bequeath for its solution to posterity.

We have occupied so much of our space with the general principles of epidemic pestilences that we have not time for comment upon the several examples. We may, however, be allowed to say in general, that they form most interesting and highly important dissertations, written in a very attractive style. M. Anglada will, we trust, allow us, before we lay down his work, to clear up a slight historical error which occurs in his chapter upon the English sweating sickness, as regards this country. It is there stated that Scotland and Ireland enjoyed an immunity from this epidemic. As regards Scotland, it is supported by the authority of Dr. Freind, who says, "Quod mirum est, in exteris nationibus, Scoti illæsi, Anglique tantum tentati erant, neque in Anglia peregrini afficiebantur." We have no grounds to prove that Ireland escaped, yet Dr. Anglada, on the assertion of Larrey, states, "le reste du royaume fut frappé presque en même temps, toujours à l'exclusion de l'Ecosse, et de l'Irlande," and he repeats the observation while describing the repeated epidemic returns of the disease. It is possible that on this point the author was misled by Hecker, who

has stated positively that "the sweating sickness did not advance westward to Ireland, nor did it pass the Scottish border." "The historians who would," he says, "certainly have recorded so calamitous an event are entirely silent." Nay, further, he goes on to found a theory for its restriction to the English nation. But there is abundant proof that M. Hecker's facts and theory are alike unsustained. In Hardiman's *History of Galway*, compiled from the town annals, we are told "this charitable institution (the Leper Hospital) was fortunately completed in the year 1543, when the sweating sickness broke out and raged with great violence, destroying multitudes of the natives, and particularly the tradesmen of the town." This would seem to refer to the fifth epidemic of this plague. Again, in Smith's *History of Cork* (Vol. ii, page 35), we find it stated on the authority of local MS. that in 1528, the fourth epidemic, there was "a malignant disorder called the sweating sickness in Cork." The same fact is recorded in the *Annals of the Four Masters* under the year 1492, the first epidemic of the disease, "An unusual plague raged in Meath, a plague of twenty-four hours' duration, and any one who survived it beyond that period, recovered. It did not attack infants or little children." In the Dublin copy of the *Annals of Ulster*, this is called "plaigh allais," the sweating sickness, "that sad contagion which no former age knew."

We cannot rise from our present pleasing task without cordially thanking M. Anglada for his work, which supplies what was felt to be a desideratum in our general medical literature.

JOHN POPHAM.

WORKS ON HYGIENE AND SANITARY SCIENCE.

1. *A Manual of Practical Hygiene, prepared specially for the Medical Service of the Army.* By Edward A. Parkes, M.D., F.R.S.; Professor of Military Hygiene in the Army Medical School. Third Edition. London: Churchill and Sons. 1869. Large 8vo, 640 pages.

2. *Traité d'Hygiene Generale.* Par le Docteur Adolphe Motard. Paris: Baillière. 1868. 8vo, 2 vols, 878 and 821 pages.

3. *Traité de l'Alimentation dans ses rapports avec la Physiologie, la Pathologie et la Therapeutique.* Par le Docteur Jules Cyr. Paris: Baillière et Fils. 1869. Large 8vo, 578 pages.

4. *Disinfectants and Disinfection.* By Robert Angus Smith. Ph. D., F.R.S. Edinburgh: Edmondston and Douglas. 1869. 8vo, 138 pages.

The first two editions of Professor Parkes's work on *Hygiene* have been so fully described in this Journal that the task of reviewing a third issue is a light one, more especially as its immediate predecessor was published only three years ago. It is not, however, altogether because the great demand for this work speedily exhausted the second edition that a third one so soon followed, for so zealously is sanitary science now cultivated that even the comparatively brief period of three years produced no inconsiderable amount of work "to be written up." Dr. Parkes takes care to keep himself thoroughly and intelligently posted up in reference to the labours of sanitarians throughout the world, and everything that is really worthy of record, or likely to be of the least practical use, he presents to us in his admirable work.

Of the former edition of Parkes's *Hygiene* we have written in the most commendatory terms, and of the present we can truthfully aver that it is by far the most valuable treatise on sanitary science in the English language, and that it is—taking into account its special military application—quite equal to the best treatises of the same subject to be found in France or Germany. In this work we find not only the most recent information, but every doubtful statement is critically examined and placed in such a way before the reader as to enable him to judge of its value. Indeed, as a rule, the pages of the work are not encumbered with mere theories and hypotheses, but are filled with substantial facts. If a process for purifying water, or for disinfecting foul air, is given, Dr. Parkes tells us to what extent we may rely upon its efficacy, whether or not the process is sure to give good results, and under what conditions. As a rule, writers on sanitary science are far too inexact in their figures and conclusions. They write with far too great a degree of positivism. Into this fault Dr. Parkes has not fallen. He recommends various sanitary agents, but at the same time he tells that some of them often fail, even when they are most likely to prove useful. His book is in every respect a candid one, and he claims no more for the science of which he is so zealous a cultivator than that which is indisputably due to it.

The third edition is not much larger than the first one, although it contains more information. This is accounted for by the great

condensation in the mere verbiage of the book, which distinguishes the present edition. Not one word is wasted, and in almost any fifty pages of this manual more substantial information will be found than in many a bulky work on science. In no place, however, is brevity carried so far as to render the meaning obscure. A more intelligible or more correctly worded volume we have never read.

The general arrangement of this edition is similar to that of its predecessors, but several chapters have been entirely re-written. The most striking alteration is the adoption of the new notation in chemistry, a change which, we fear, will prove somewhat embarrassing to medical readers who "took out their chemistry" from six to thirty years ago. The new notation is now almost universally in use amongst pure chemists, and it is gradually finding a place even in the chemical language employed in the medical journals. We notice, however, that Dr. Parkes uses, where he can, the common names of chemicals—for instance, oil of vitriol, or, even more learnedly, sulphuric acid, instead of sulphuric hydride.

A large proportion of Parkes's *Hygiene* is occupied with matter solely of interest to military men. We now venture to suggest to the author the desirability of preparing an edition of his excellent work adapted for the use of the civil population. About three-fourths of the existing work would be available for this purpose, and if the remaining fourth part were replaced by a few chapters on sanitary legislation, including a digest of the Sanitary Act of 1866, a work of great value to the general public would be the result.

Our French neighbours have long been distinguished for their writings on hygienic and diatetic subjects. The comprehensive works of Levy, Tardieu, Donne, Fonssegrives, and many other authors, prove how highly appreciated are the labours of sanitarians in France, and how industriously hygienic science is cultivated in that country.

M. Motard's work is of a very comprehensive character. There is hardly a subject in any way relating to public or private hygiene which is not fully discussed in it. The work is divided into seven parts, or books, each of which is subdivided into a large number of chapters. The first book is, to some extent, of an introductory character, and is the most theoretical portion of the work. A large portion of it is devoted to that most recondite and unsatisfactory subject—the origin of man. This delicate topic is, however, very carefully treated, and the author takes by no means a purely

materialistic view of the intellectual part of human nature. He justifies that artificial arrangement in the scientific classification of animals, which assigns to man a separate and independent kingdom. The rest of this book is devoted to the consideration of the anatomy and chemical constitution of man, the physiological uses of the more important organs, the influence of the physical state of the body upon the mind, and the doctrine of temperaments.

The second book treats altogether of climates, and is illustrated by numerous maps. This part of the book is so very scientific that minute instructions are given in it for the quantitative analysis of the air. If one wants to know the mean minimum or maximum temperature of the air and water in almost any part of the civilized world, the information may be readily found in this book. As might be expected, the influence of soil upon the public health is very fully dwelt upon, though we were rather surprised not to find some reference to the remarkable facts relative to the improvement of the health of English towns by thorough drainage, which have been recorded in late volumes of the Annual Reports of Dr. Simon, the medical officer of the Privy Council. Relative to potable waters, Dr. Motard writes very fully, largely quoting the results and opinions given by British chemists and physicians. He inclines to the view that most zymotic diseases are more or less circulated by means of potable water.

The third book may be regarded as a complete treatise on nutrition. It describes the nature and preparation of foods of every kind, enters elaborately into the consideration of their influence upon both the body and mind of man, and discusses at great length the uses and abuses of condiments, alcoholic liquors, and other stimulants. This book, and the two which we have already referred to, constitute a bulky volume of 876 pages, a fact which shows how exhaustively every subject has been dealt with.

The second volume consists of four books, the first of which is devoted to clothing, ablutions, and exercises; the second to labour, or the work of man in the arts of peace and war; the third to prophylactics, or the nature and prevention of zymotic and other diseases, which are to some extent under man's control; and the concluding book is wholly taken up with the social conditions of civilized communities—education, justice, police, &c.

We have preferred to give a brief statement of the whole contents of this excellent work rather than to describe any portion of it fully in detail, for it is not a book which admits of extracts. It

is, indeed, a kind of encyclopedia of sanitary science, and one which should be found in the library of every one who devoted himself to the solution of problems relating to the health and well-being of the community. It has been said of Kepler that he was a great mathematician, but a bad philosopher. We may say of M. Motard, that we hardly know in which character—that of a philosopher, or that of a mere scientific writer—he is most deserving of our commendation. A more accurate, complete, and elegantly written work of the kind has never come under our notice; and we can conscientiously say, that having carefully read it through, we were unable to find any statements which could fairly form the subject of hostile criticism.

Dr. Jules Cyr's work is a treatise on "Food and Diet," which we should be very glad to see in an English form. In some respects it resembles Dr. Moleschott's celebrated work on food and diet, of which a defective partial translation into English was made a few years ago. It is, however, a much more practical treatise than Moleschott, and one which is much more likely to prove useful to the physician.

The greater part of this work is of course devoted to the description of the chemical composition, and relative nutritive values of all the more commonly employed foods. We find also much space devoted to such subjects as the preservation of aliments, the influence of cookery upon food, &c. The more important portion of the work is, however, that which treats of the use of food in disease, and of the diseases produced by the use of unsound and diseased foods. The author speaks very favourably of the advantages resulting from the use of a milk diet. He quotes numerous authorities in support of this view; amongst others, Dr. Karell, who states that in dropsical affections, phthisis, diseases of the liver, and in diseases of the stomach and intestines, following affections of the nervous centres, he considered the milk cure as the first of remedies.

With reference to the diseases produced or induced by the use of bad water, Dr. Cyr states, that goitre and cretenism are probably the result of an excess of a particular kind of organic matter in that liquid. The hypothesis that excessive quantities of earthy (chiefly magnesia), salts are the cause of these repulsive diseases, is not yet exploded, and the arguments in favour of that hypothesis are certainly much stronger than those which, according to Dr. Cyr, should lead us to infer that organic matter in water produced goitre.

We were not a little gratified to find in Dr. Cyr's works, many references to the labours and opinions of Irish physicians, particularly to Graves "le célèbre professeur de Dublin," and the Rev. Professor Haughton.

We shall conclude this notice of recent works on hygiene by a brief reference to Dr. R. Angus Smith's little book on *Disinfection*. It consists of a few chapters, in each of which is described the preparation, mode of application, and supposed efficiency of one or more of the substances used for disinfecting purposes. The author praises sulphurous acid, nitrous acid, &c., but his perfect type of a disinfectant appears to be carbolic acid and its compounds. A large proportion of the contents of the book is taken up in describing experiments with the latter disinfectant.

Dr. Smith may be regarded as one of our best, if not the best, of our chemical authorities on disinfection, and we are therefore disposed to pay great respect to his opinions relative to the remarkable sanitary properties of carbolic acid. Our experience of that agent is most favourable, though in the case of the disinfection of the air of sick rooms, we should depend more upon the action of nitrous acid, than of carbolic acid, vapours. We think Dr. Smith might not have taken up so much of his work with mere details of experiments with carbolic acid—the results would have been sufficient. He might also have given more precise information relative to the application of the various disinfecting agents, so as to render his book of practical utility to medical officers of health, and physicians who are not skilled in chemical manipulation. Indeed Dr. Smith would have conferred a boon upon the public had he given us a cheap and handy little book on disinfection, containing plain directions for the purification of air, water, and clothing. We fear the book under our notice, excellent though it undoubtedly is, will not be largely purchased by the class for whose benefit we presume it has been written.

WORKS ON ELECTRICITY.

1. *Electricity in its Relations to Practical Medicine.* By Dr. Moritz Meyer. Translated from the third German Edition, with Notes and Additions by W. A. Hammond, M.D. New York: D. Appleton & Co. 1869. 8vo, pp. 497.
2. *A Treatise on Medical Electricity.* By Julius Althaus, M.D. Second Edition. London: Longmans, Green & Co. 1870. 8vo, pp. 676.

Of the standard works in German devoted to medical electricity, viz.: those of Remak, Rosenthal, Ziemssen, Benedikt, and Meyer, the last alone, so far as we know, has appeared in an English dress, and thus become accessible to a wider circle of readers. The Americans have, in its translation, as well as in many other instances, outstripped us in their zeal to bring the writings of the best continental authors within the reach of all. An acquaintance with the groundwork of the physics of electricity, and with the principles of electro-physiology is an essential preliminary to the rational employment of electricity in diagnosis and prognosis, or as a curative agent, and no practical physician can now in justice to himself, or in fairness to his patients, afford to neglect the help to be derived from electricity. An electrical apparatus, of one form or another, will soon become as necessary to a physician's equipment as a stethoscope or a thermometer, and as a complete guide to all who would become conversant with the present state of electro-therapeutics Dr. Meyer's treatise can be safely recommended.

The American edition leaves nothing to be desired as to its external attractions, and though we cannot say quite as much of the merits of the translation, Dr. Hammond has enriched it with some important notes, especially under the head of Infantile Paralysis, to which he has paid considerable attention.

Static electricity as represented by the ordinary plate or cylinder machine, and the Leyden jar, has for some time practically been abandoned, perhaps not quite deservedly, and the most important question now to be decided is the relative value of the direct, or constant, and the induced current.[a]

[a] The term "induced current" is not open to the ambiguity which attaches to the phrase "interrupted current," often used as synonymous with it, for it is plain that a galvanic current (*i.e.*, direct battery-current) can equally well be interrupted, and moreover induced or faradic currents do not operate by virtue of the interruption, but the essence of their mode of action is that they are *momentary* currents which quickly rise from zero to a maximum, and *vice versa*, so that a very rapid change of density is brought about.

Until a sufficiently permanent and portable battery for furnishing the direct current is provided, the superior convenience of the small and elegant induction apparatuses now made will ensure their more general adoption. But as it is clearly a fundamental point to come to a distinct understanding as to the respective functions of the induced and the direct current (the so-called constant or continuous current), we will briefly abstract some of the leading conclusions at which Dr. Meyer arrives:—

1. The *interrupted* current is applicable in those cases in which we wish (*a.*) to excite the muscles, the nerves of special sense, the sensary or motor nerves; (*b.*) to produce contractions of the blood or lymphatic vessels; (*c.*) or to affect certain organs supplied by the sympathetic.

2. The temperature and volume of a muscle are increased by the contraction produced by Faradization.

3. An increase of sensibility is effected by currents of tolerably quick intermission, and not exceeding a certain duration and intensity; under opposite conditions there is soon a marked decrease in sensibility. Hence we should only use currents of such strength and duration as are sufficient to produce the desired result.

4. If we wish to relax a tense muscle, repeated intermissions of a strong current, or single shocks from a strong induction current are generally more efficacious than the constant current.

5. The use of the *constant* current is indicated in those cases in which we wish—

(*a.*) To excite the nerves of special sense or the cutaneous nerves.

(*b.*) To destroy the skin or mucous membrane.

(*c.*) To produce an increase of warmth.

(*d.*) Or to induce a chemical process, *e.g.*, coagulation of blood.

6. While, as a rule, the induced current furnishes the most powerful means for exciting the motor nerves and muscles, there are certain peripheric paralyses in which the constant current, probably in consequence of its longer duration, produces effects which cannot be brought about by the induced current.

7. The sensitiveness of a muscle to the induced current can, in many cases, through the application of a tolerably strong constant current, be increased.

8. In galvanizing a nerve it is advisable frequently to change the direction of the current, because the conductivity for a current, flowing long in the same direction, decreases, while with a change in the direction it increases.

12. There is no doubt that we can, by means of a constant current, even of tolerable strength, affect through their bony coverings the brain and spinal cord.

The paragraphs relating to the electrotonic state are omitted. In the opening sections of the book a brief sketch is given of the history of medical electricity, and of the different forms under which electricity is manifested, followed by an outline of animal electricity. The action of the electric currents on the different organs and tissues is more fully discussed, and a detailed description is given of some of the various kinds of apparatus designed especially for therapeutical purposes.

The method of using the induced and constant current with precision is described, and a section is devoted to the application of Electricity to Anatomy, Physiology, and Pathology. Of more interest to the physician is the direction of electricity to the diagnosis and prognosis of paralytic affections, in which class of diseases it can render most important aid, and the author having thus fully treated the *principles* of electro-physiology, enters at length, and with thoroughness, into the *art* of electro-therapeutics as illustrated in medicine, surgery, and obstetrics.

The number of editions through which Dr. Meyer's work has passed in Germany attest the esteem in which it is there held, and we do not doubt that an equal measure of approbation will be accorded to it in this country.

It is with great pleasure that we welcome, and cordially recommend Dr. Althaus' treatise, in the improved guise of a second edition. Dr. Althaus' work possesses the merit of being clearly and agreeably written, while its teaching is in accord with the most recent information; and the balance is evenly held between the relative virtues of galvanization, and faradisation—a point of the highest importance in the treatment of disease. Among the more recent positive gains to medical electricity, we can reckon the right appreciation of the use, and even indispensableness of the direct current in certain cases of paralysis; the more certain cure of severe neuralgia; the undoubted possibility of directly galvanizing the brain, and spinal cord; and the galvanic irritation of the sympathetic nerve.

These as well as many other questions of great interest and importance, such, for example, as the electrotonic action of the direct current on living nerves, are carefully discussed in the work before us.

One of the subjects to which Dr. Althaus has directed special attention, and on which he is entitled to speak with authority, is the electrolytic treatment of surgical diseases. Three years ago he had already published a pamphlet on the electrolytic treatment of tumours, and shown that the proper mode of procedure was to introduce one or more needles connected with the *negative pole*, into the depth of the tumour; while a moistened conductor, *e.g.*, a sponge, was connected with the positive pole, and placed on the skin adjacent to the tumour. This method has been successfully applied to aneurism, nævus, bronchocele, sebaceous tumours, hydatid cysts of the liver, &c.; and it is probable that we do not yet know the full extent of its powers.

It is most suitable, and will, in all likelihood, find its chief use in the dispersion of tumours containing fluids, especially, serous cysts, and effusions; and we have succeeded in rapidly dispelling, by this means, a ganglion of the wrist, which had existed for a great length of time.

To the favourable opinions already accorded to the former edition of this treatise, we can add nothing, except in the way of sincere commendation; and to Dr. Althaus belongs the credit of having filled up a hiatus in English medical literature, by the production of a sound, comprehensive, and practical work, on the right use of an agent, whose powers are daily becoming better recognized, and more clearly defined.

A System of Surgery, Theoretical and Practical, in Treatises by Various Authors. Edited by T. Holmes, M.A., Cantab.; Surgeon and Lecturer on Surgery at St. George's Hospital. Second edition. London: 1870. Vols. I. and II.

In noticing the publication of the first two volumes of the second edition of *Holmes' Surgery*, we have to congratulate the editor on the success of his first edition, and on the many improvements on it which are noticeable in the present issue. The arrangement of the text has been extensively altered; many passages which were printed as notes in the former edition being introduced into the text, which has itself been revised. The first edition contained no illustrations; the present is illustrated both by coloured lithographs, and with woodcuts inserted in the text, an addition which will greatly increase the usefulness of the work. The illustrations, the

editor informs us in his preface, have been supplied as freely as was possible, "without unduly increasing the cost of the work." The paper on Inflammation, by Mr. Simon, has been materially altered in the present edition by the postponement of the discussion of the pathological theories of inflammation to the promised fifth volume of the work. The object of this change was to allow time to elapse before the consideration of questions which are at present the subject of the utmost controversy, resulting from the publication of Cohnheim's views. We hope, though our hope is far from sanguine, that the publication of the fifth volume will set at rest our uncertain ideas on this subject. Mr. Croft's paper on Hectic and Traumatic Fevers has been transferred from the fourth volume to the first, and has been illustrated by thermographs of the changes observed in these forms of fever, which will be studied with interest. The observations of Billroth, which have been repeated and confirmed by Mr. Croft, show that the highest temperature reached in cases of traumatic fever does not exceed F.104·9°, and that under favourable circumstances the fever begins to decline on or after the second day; cases which attained the maximum temperature on the third or fourth days were, with a single exception, attended by some complication. The thermographs obtained in hectic fever show well the remittent character of the fever. In these cases a temperature about F.102° is exceptional, and sudden exceptional falls and rises of temperature indicate the occurrence of complications. We look forward with anxiety to the publication of the remaining volumes of this excellent work, more particularly to the fifth, which promises to contain so much new and important matter.

The Body and its Health, with Teachers' Aid and Questions. By E. D. MAPOTHER, M.D., Professor of Physiology, Royal College of Surgeons; Surgeon to St. Vincent's Hospital, &c. Third edition. Dublin: John Falconer, 53, Upper Sackville-street.

THE favourable opinion which we formed of Dr. Mapother's book has been endorsed by the public, and already a third edition has been issued. Along with it may be had questions, which those engaged in teaching elementary physiology will find admirably suited for ascertaining how far their pupils have comprehended the lessons contained in the body of the work.

Nouveau Dictionaire de Médecine et de Chirurgie Pratiques Illustré de Figures intercalées dans le Texte. Rédigé par Benj. Anger, E. Bailly, A. M. Barrallier, Bernutz, P. Bert, Bœckel, Buignet, Cusco, Demarquay, Denucé, Desnos, Desormeaux, A. Desprès, Devilliers, Alf. Fournier, T. Gallard, H. Gintrac, Gombault, Gosselin, Alph. Guérin, A. Hardy, Heurtaux, Hirtz, Jaccoud, Jacquemet, Jeannel, Kœberlé, S. Laugier, Lannelongue, Ledentu, Liebreich, P. Lorain, Lunier, Luton, L. Marchand, A. Nélaton, A. Ollivier, Oré, Panas, M. Raynaud, Richet, Ph. Ricord, Jules Rochard (de Lorient), Z. Roussin, Saint-Germain, Ch. Sarazin, Germain Sée, Jules Simon, Sireday, Stoltz, A. Tardieu, S. Tarnier, Valette, Verjon, Aug. Voisin. Directeur de la rédaction: le docteur Jaccoud. Paris: J. B. Baillière et Fils.

The eleventh volume of this *New Dictionary of Medicine and Surgery*, in large octavo, has now appeared, each volume containing about 800 pages, and it brings us only to the end of the fourth letter of the alphabet; from this some idea may be formed of the extent of the work. To many of the articles we shall have occasion to refer from time to time, and at present, therefore, we merely draw attention to the general character of the undertaking. So far as he has gone, the editor seems to have omitted no subject connected with the practice of medicine and surgery. The reader will find discussed as fully as in the special treatises questions in anatomy, physiology, pathology, chemistry, botany, pharmacy, hygiene, state medicine, dietetics, and midwifery, as well as in practical medicine and surgery. Innumerable matters on which he would find it impossible elsewhere to obtain information, are here considered. The articles, as far as we have been able to read them, are excellently written; they are abundantly illustrated, and all the important ones are accompanied with most valuable bibliographical indices; indeed, the writers seem to have been instructed to deal exhaustively with the subjects committed to them; sometimes the wish to do this has led them to enter into matters of historical, literary, or speculative interest, with a minuteness of detail which most readers would not desire. The production of such a work as this is a gigantic enterprise; we do not know of any similar one which approaches to it in completeness; we have no hesitation in saying every medical library for reference, should possess it, and

we have little doubt it will soon be found in every public one throughout the civilized world. Every honour is due to those who have projected it, and are carrying it to completion.

Nouveaux Elements d'Anatomie Chirurgical. Par BENJAMIN ANGER, Chirurgien des Hopitaux, &c.

THE chief merit of this work lies in the illustrations of normal and morbid anatomy, which are intercalated in the text; while the volume contains 1,056 pages, the text contains 1,079 illustrations, all of which are admirably executed.

The work is divided into two sections, the first of which is devoted to general surgical anatomy; the second, to the anatomy of the regions of the body. Under the head of general anatomy, the subjects of the embryology and the anatomical elements of the blood and of the tissues are examined : the second part devoted to the consideration of regional anatomy, contains excellent representations of the normal anatomy of each region; with a clear and concise descriptive text. In both sections the description of each tissue and region, concludes with an illustrated account of the morbid anatomy of each element and structure.

In the attempt to condense so much matter into a single volume, many subjects are necessarily treated imperfectly, and many omitted entirely; but these defects are compensated by the excellence and great variety of the illustrations of those treated of more fully. Many of the omissions which we have observed, are difficult to explain; such, for instance, as the omission of any detailed description of fractures of the clavicle, or illustrations of them; while many of the corresponding injuries of the humerus are depicted and described. We infer that the author has been forced to omit many such subjects, in order to keep the work within its present compass, as we are aware that he has in another work very fully figured these injuries. Again, in the description of the surgical anatomy of the humerus, we fail to find any account of the anatomy of its epiphyses, or any but the slightest allusion to their separation by injury; none whatever of the separation of the lower epiphysis, while the diagnostic signs of fracture of the lower extremity of the adult bone, and of the luxation of the bones of the forearm backwards are considered in detail. Many such omissions occur, and necessarily diminish the value of the work as a text book; but we know of no other which contains, in the same space, so great a variety of excellent pathological drawings.

PART III.

QUARTERLY REPORTS.

REPORT ON MEDICINE.[a]

By James Cuming, M.A., M.D.; Professor of Theory and Practice of Medicine, Queen's College, Belfast; Physician to the Belfast General Hospital; Examiner in Medicine in the Queen's University in Ireland.

RECENT OBSERVATIONS REGARDING THE NATURE OF PULMONARY CONSUMPTION.

Several questions of the highest importance have been recently discussed both in this country and abroad in connexion with the subject of pulmonary consumption. Of these the one possessing most practical interest is whether phthisis is to be regarded as being always of a tubercular nature, or whether it is engendered by ordinary morbific causes independently of any specific form of lesion.

A few years ago the invariably tubercular character of phthisis was one of the most unquestioned propositions in pathology. Physicians accepted this view of the subject on the authority of Laennec as an axiom in medical science, and although the doctrines of this illustrious observer were hotly disputed in his own time, they ultimately came to be universally adopted almost without question. Within the last few years, however, very serious attacks have been made on the prevalent belief, and a large amount of clinical and pathological evidence adduced in favour of the doctrine that the etiology of phthisis cannot be summed up in the word tubercle, but that this wide spread and terrible malady may be and often is engendered in individuals who are not tuberculous, and who never become so.

That this is not a mere question of pathological theory will be at once evident. If consumption be always the result of a diathesis, and if the lung affection be only the material evidence of the dyscrasia, then the prophylaxis and management of the disease

[a] The author of this Report, anxious that every contribution to Pathology and Practical Medicine should be noticed, will be glad to receive any publications on these subjects. If sent to correspondents of the Journal they will be forwarded.

must be conducted on different principles to those which we should be obliged to adopt if the disease were likely to be brought about by a neglected cold, as is popularly believed, or by a pneumonia or hemoptysis. Accordingly it becomes a question of great significance how far we are justified in regarding the well-known symptoms and physical signs of consumption, and the destructive changes in the lung in connexion with them, as always produced by tubercle.

That pulmonary diseases exist, unconnected with tubercle, which present local and constitutional manifestations closely resembling those of phthisis, has been long known. Cirrhosis of the lung, for example, is a wasting disease attended by cough, expectoration, hemoptysis, and by physical signs very like those of phthisis. The more recently described fibroid phthisis is another non-tubercular affection of a similar character. In cases of this malady which has been well described by Dr. Sutton[a] and others, it has been found quite impossible to establish the diagnosis, from tubercular phthisis merely by stethoscopic examination. The changes in the lungs which, it is to be noted, are found especially in the upper lobes are, however, merely the result of fibroid degeneration, the newly formed fibroid tissue in some parts, completely destroying the pulmonary parenchyma. There are also several varieties of what is called mechanical bronchitis, such as the pulmonary affections produced in knife-grinders, stone-cutters, coal-miners, and in individuals pursuing several other trades, the general exciting cause being the inhalation of minute particles which act as mechanical irritants, and the symptoms and physical signs being in many instances identical with those of phthisis. Two remarkable cases recently recorded by Zenker[b] put the nature of this malady in a very clear light. In these cases the disease was caused by the inhalation of oxide of iron, this substance in fine powder being used in the manufacture of little books in which leaf gold is placed. The air of the room in which the patients worked was actually coloured by the dust of the iron, and the walls and furniture were covered with it. The lungs of two persons who had worked in this atmosphere presented a remarkably red tint, owing to the presence of the iron in them, and the lungs of one patient were found, on chemical examination, to contain the extraordinary amount of above 320 grains of this substance.

[a] Medico-Chirurgical Transactions, Vol. xliii, p. 287.

[b] Deutsches Archiv. fur Klinische Medicin, band 2, S. 116.

It had been frequently disputed whether the black discolouration occasionally met with in the lungs is owing to pigment derived from the blood, or to minute particles of carbon which had been inspired with the air, and the question had been asked by Friedreich, why it never happens that red discolouration of the lung is found. Zenker's cases remove any doubts that may have existed on this subject.

In one of these cases that of a woman, aged thirty-one years, both lungs exhibited on section a number of small nodules, some of which were altogether solid, others had a small opening in them corresponding to a very small bronchial tube. All the lobes of the lungs with the exception of the middle lobe of the right lung contained irregular cavities of various sizes.

Dr. Zenker remarks that this case would formerly have been unhesitatingly described as one of tubercular phthisis. He argues that this view is untenable, because nothing presenting the anatomical or histological characters of tubercle was found in the lungs, and no tubercles were met with in any of the other organs. The indurated nodules he regards as the result of lobular interstitial pneumonia, and the cavities as formed in consequence of an ulcerative process commencing in the bronchial mucous membrane, and after penetrating the wall of the bronchial tube involving the pulmonary parenchyma.

Dr. Greenhow[a] gives an account of the morbid appearances in several cases of lung disease arising in colliers, miners, and flax-dressers. In one of the best marked of these cases, a large irregular ragged cavity existed. The general conclusions of Dr. Greenhow are as follows:—

"The nature of the substance inhaled appears to be of secondary consequence as regards the ultimate result, excepting that the heavier and more penetrating kinds of dust, such as angular particles of grit, more speedily excite serious disease than the lighter kinds. Mechanical particles inhaled into the air-passages set up, in the first instance, more or less irritation of the bronchial membrane, which may entirely subside again if exposure to its exciting cause be discontinued. On the other hand, a continuance of the mechanical irritation rarely fails sooner or later to induce those changes in the interstitial tissue of the lungs seen in the several specimens which I have exhibited to the Society on the present and former occasions.

"The characters common to all these cases are that the pleura is

[a] Transactions of the Pathological Society of London, 1869, p. 41.

generally somewhat thickened over a greater or less extent, and that pleural adhesions are frequent. The density of the lungs is generally more or less increased, and patches of consolidation of various sizes are found in their substance. Sometimes the greater part of a lobe is so consolidated; but more frequently the consolidation is of smaller extent, and is distributed throughout the lungs in several, sometimes many, well-defined hard nodules, varying in size from a hemp-seed upwards to that of a walnut, or even larger. The consolidated tissue, whether forming small nodules or occupying the greater part of a lobe, cuts with a smooth dry section, and, unless where the orifices of divided bronchial tubes are visible, presents no resemblance to ordinary lung tissue, all traces of the vesicular structure being obliterated. On section, the nodules are sometimes of an iron-grey or greyish-yellow colour, but more commonly black, or nearly so, from the abundant deposit of pigment in their tissues; in many cases they are seen with the naked eye, and in almost all cases with the microscope, to be intersected by bands or lines of fibrous tissue. When the smaller nodules are cut across, the orifice of a divided bronchial tube is generally seen in the middle of the nodule, showing apparently that the consolidation has commenced immediately around the tube. The bronchial tubes in the consolidated tissue are frequently dilated and thickened, and are seen to stand out prominently above the level of the cut surface, surrounded by a black dense case, consisting of the indurated and deeply pigmented tissue immediately around the tubes. When the tube is of small size, it is only on close examination that its orifice can be discovered in the centre of the black projection. Even those parts of the lungs which are still crepitant are for the most part denser and less elastic than normal lungs, and they are also very frequently traversed by fibrous bands or tracts similar to those seen in the nodules. In some specimens these tracts are narrow and not very visible to the naked eye, but in others they are broad and very obvious. When cut into, the lungs usually exude more or less of a black fluid, which closely resembles Indian ink in appearance. This fluid is most abundant in the lungs of miners, from the crepitant portions of which it often pours out in large quantities. When evaporated to dryness, it leaves a black residue which is quite insoluble by boiling in strong nitro-hydrochloric acid."

Do other morbid processes occur of less obvious origin giving rise to the familiar characters of pulmonary consumption?

Dr. Charles Theodore Williams[a] contributes some important facts relative to the causation of consumption, mainly drawn from the experience of Dr. C. J. B. Williams. Out of 800 cases

[a] "The Causes of Pulmonary Consumption," by C. T. Williams, M.D.—St. George's Hospital Reports, 1869, p. 153.

tabulated from the private practice of the latter gentleman, on the ground that each patient had been at least one year under observation, it was found that 101 or about one-eighth originated in bronchitis and influenza; of these 47 were free from hereditary predisposition. Out of the same number 121 cases could be traced to attacks of pneumonia and pleurisy, and in 69 of these there was no evidence of hereditary predisposition. It is probable that an even larger proportion than this of cases of consumption originating from local causes would be found among the poorer classes, inasmuch as these figures have been drawn from the private practice of an eminent London physician.

Perhaps the most distinguished advocate of the doctrine, that consumption in many cases owes its origin to other than tubercular causes is Professor Niemeyer[a] of Tubingen. It is mainly to the writings of this eminent physician that the spread of this opinion is owing. Niemeyer has collected a large amount of both pathological and clinical evidence to show that consumption is often produced by chronic inflammatory processes in the lungs, the affection which, in his opinion, most frequently engenders it being chronic catarrhal pneumonia. This disease is usually preceded and accompanied by bronchitis—from the bronchi the morbid process extends to the air-cells, so that these as well as the smallest tubes become filled with the products of the catarrhal inflammation. These are the cells and fluid usually met with in inflammations of the mucous membranes, and as the fluid becomes absorbed, the solid elements undergo the caseous or cheesy degeneration—a process which plays a very important part in the theory of consumption, and which ultimately leads to softening of the pulmonary tissue.

Other cases of consumption take their origin according to the same observer in hemoptysis. We have been in the habit of believing that pulmonary hemorrhage is in the majority of cases an evidence of the existence of tubercle, and not a cause of phthisis. According to Niemeyer, of those who have hemoptysis a considerable number are not phthisical, and never become so. The effused blood in some instances excites the inflammatory process, giving rise to lobular pneumonia, the portion of extravasated blood remaining in the lung sharing in the same process of degeneration as that which the inflammatory products undergo. In cases in which this

[a] Berlin Klin. Woch. Nos. 17 & 18, 1869. Text-book of Practical Medicine. Trans. by Humphreys and Hackley. New York, 1869.

effect is produced by hemorrhage, Niemeyer has found that, in about three days after the attack, febrile symptoms manifest themselves, the temperature rises, and pain is felt in some part of the chest. In this part fine râles become audible, the respiratory sounds are altered, and pleural friction is occasionally manifest, along with these is dulness on percussion.

In some instances hemoptysis is secondary to an inflammatory process already existing in the lungs, a sequence which can be recognized by observing that the febrile condition, and the other evidences of inflammation accompany or precede the appearance of blood in the expectoration. Another source of danger from hemoptysis consists in the absorption of portions of the degenerated blood, and consequent infection of the system.[a]

If we turn now to the pathological evidence and inquire what grounds there are for supposing that the destructive changes in the lungs met with in *post mortem* examinations of persons who have died of pulmonary phthisis are always of a tuberculous nature, we find that the great point to be decided is, what is the nature of the yellow cheesy substance found in the lung, and which Laennec designated as crude yellow tubercle. Without entering into unnecessary details we may say that the progress of microscopical observation enables us to state with certainty, that this substance is in many instances not tubercle at all. A material altogether undistinguishable from this so-called tubercle is produced by the drying-up of pus, by the degeneration of lymphoma or syphilitic deposits, and is even found in cancerous growths as well as under various other circumstances. In some cases this material is undoubtedly tubercle, and the classical description of Laennec as to the change produced in the gray granulations has been too often verified to be seriously disputed. But the only unquestionable evidence of the existence of tubercle consists in the presence of the gray granulation, and not in that of the yellow substance which may be tubercle, but which may also be one of a number of other things; nor in the existence of cavities, which, as we have seen, may result from very different causes.

Even when gray granulations are present, Niemeyer holds that they are in many cases secondary to other pathological changes.

[a] In a very interesting memoir which has been translated by Dr. W. D. Moore, Dr. Rasmussen, of Copenhagen, has detailed a number of cases of hemoptysis in which aneurisms of small branches of the pulmonary artery existed, which had by their rupture given rise to the hemoptysis. This condition has now been found by the author in eleven cases.—Edinburgh Medical Journal, August, 1869.

We shall have occasion to explain, in connexion with the origin of tubercle, how this secondary tubercular formation is supposed to be brought about.

When the now celebrated experiments of Villemin, of the results of which an account was given in this Journal in August, 1868, were published, the possibility of the artificial production of tubercle by inoculation, was soon definitively established by the labours of a large number of experimentalists. That tubercle can be generated in an animal by the inoculation of tubercle can now be no longer called in question, consequently infective powers must be conceded to exist in it. But there seems equally little doubt that tubercle may be generated by the introduction into the system of other substances, even of inorganic bodies as well as by the irritation of setons and the like. Of this fact we have the concurrent testimony of many observers. The experiments of Cohnheim and Fränkel among others, the results of which were noticed in this Journal for May, 1869, point clearly to the origin of tubercle from morbid products which have been generated within the body. These observers found that the introduction into animals of substances, such as caoutchouc, was followed by the development of tubercle in distant organs, the necessary intermediate step being that inflammation and abscess should be excited by the substances so introduced. In instances of this kind it is clear that the organism must have become infected by morbid materials generated within itself, the altered pus being the only source to which we can refer for our explanation of the subsequent contamination. This is the process of self-infection, and it possesses great interest for the physician, inasmuch as it points to a ready explanation of various cases of consumption occurring in the human subject. It is reasonable to suppose that in man as in animals a development of miliary tubercle may be the result of the absorption of some of the products of inflammation of certain parts, when these products have reached a particular stage of degeneration. This is of course merely a hypothesis, and one which is hardly susceptible of positive proof, but it has many analogies in its favour, and it affords an intelligible explanation of serious pathological difficulties. At the same time it must be borne in mind, that when tubercle itself is employed in inoculating animals, it acts with much greater certainty in the production of tubercle than any other species of irritation or inoculation. So distinct is this difference that it establishes a probability of the specific and virulent character of

tubercle, and has led Villemin and others to the adoption of the view popularly entertained both in this country and abroad, of the contagious character of tuberculosis.

This leads us to the great practical question of how consumption is likely to be engendered in man. If we accept Niemeyer's doctrine, we must admit that bronchial catarrh and catarrhal pneumonia, are in many instances the causes to which it owes its origin. We can also admit that direct mechanical irritation of the bronchi may be the starting-point of processes which ultimately lead to consumption. We may incidentally allude to the effect of changes in gummatous syphilitic products in the same direction. All these are susceptible of direct proof, and resemble well-known processes in the organism.

Even when we come to elements of causation, for which we have to seek for proof in experiments on animals, the evidence, although weaker, is still enough to establish a good deal of probability in favour of certain modes of production.

We can readily understand that close and long continued relations with individuals suffering from phthisis, may have a direct effect in generating the disease. It is easy to comprehend that the air surrounding phthisical patients, may become at times the vehicle for transmitting to the lungs of those who are in their neighbourhood, some particles which have been brought up by the attacks of cough to which the sufferers are subject, and that in this way a direct infection analogous to an inoculation may be produced. It is of course out of the question that direct inoculation should be practised, but it is possible that what is called self-infection may be not unfrequently brought about. With regard to this clinical observation comes to the aid of experiment, and points out that in a large number of cases of consumption, some focus of infection can be ascertained to exist, for example, enlarged and degenerated lymphatic glands, purulent exudations in the pleura or in the peritoneum which have been dried up, and other similar conditions in different organs in which cheesy materials have been formed, and are retained in the system as the result of pathological processes. The general fact being that infection of the system is likely to be produced by absorption of material from any part where chronic inflammatory changes have gone on, and that this absorbed material is likely to produce a morbid process in the organs to which it has been carried. It is worth noting that the diseased organ from which the infecting

material is derived may be the lung itself, and that very frequently a chronic pneumonia may become the starting point of the infective change. This is the explanation of the apparent paradox of Niemeyer, that the greatest danger to which a consumptive patient is exposed is that he may become tubercular.

The infecting material is probably molecular, and is certainly in a very fine state of subdivision, and it is carried by the lymphatics, and by the veins so as to reach the tissues. It is worth mentioning that recently Lebert has drawn attention to a curious connexion existing between congenital stenosis of the orifice of the pulmonary artery, and tubercular disease of the lung, a fact which is of interest when we remember the comparative immunity from tubercle enjoyed by sufferers from those heart diseases which lead to congestion of the lungs.

It is impossible to explain how it occurs that the effect of this impregnation of the system with the products of its own diseased action should give rise to tubercle. It is also by no means easy to understand how it is that particular portions of the lungs should be so commonly selected for the development of morbid changes. The clinical fact of the preference of phthisis for the superior parts of the lungs would rather seem to be the effect of a species of elective affinity similar to what exists between certain morbid poisons and certain parts of the body, than of any common inflammatory process. Till some satisfactory explanation is given of this, it must be regarded as presenting a great difficulty in the way of the reception of the doctrines which we have endeavoured to explain.

A portion of the difficulty has been met in a somewhat summary fashion by statements which are certainly very startling, and of a nature which it would require much and strong evidence to induce pathologists to adopt. So far we have adopted the usual pathological view that in tubercle we have a new formation, having certain pathological relationships, but distinctive in itself. But in a paper in the *Edinburgh Med. Journal*, we find Dr. Burdon Sanderson asserting that there is nothing which is, properly speaking, specific in tubercle at all. Dr. Sanderson states that the parts which are most frequently the seat of what is at present regarded as tubercle, are those in which a particular kind of tissue is to be met with. This tissue is identical with that of the follicles of the

[a] Medical Times and Gazette, Dec., 1869.

lymphatic glands, and he proposed to designate it as adenoid tissue. According to this distinguished observer, tubercle in serous membranes is essentially an overgrowth of gland-like or adenoid structures, which are normally found in these membranes. In the lungs the process is somewhat more complex. A substance identical with this adenoid or gland-like tissue is produced in the alveolar septa, and at the same time cells multiply to a large extent in the alveolar cavities. The result of these changes is that small masses having the characters of lobular pneumonia are produced. These masses by their increase gradually coalesce with each other, they undergo in their centres the well-known cheesy or caseous degeneration, become softened and ultimately a cavity is formed. In all this, according to Dr. Sanderson, there is nothing structurally either specific or malignant, and he suggests that it would be productive of great benefit to pathological science, to get rid of the word tuberculous altogether, inasmuch as it has, in reality, no anatomical or chemical basis. Dr. Sanderson's conclusion is as follows:—

"Anatomically the cardinal fact is this: All cases of phthisis pulmonalis have this in common, that they commence with lobular induration, and end in disintegration and cavern-formation. In no generally understood sense can a case of acute miliary tuberculosis be said to be a case of phthisis. This broad definition of phthisis brings us back once more to Laennec. It was he indeed who first recognized the fixed relation between induration and disintegration as antecedent and consequent; but he unfortunately extended the notion from the tissue to his 'tuberculous material.' The conclusion to which we have now arrived by anatomical investigation is this: There is no difference structurally between the induration of phthisis and the induration of chronic pneumonia. There are no anatomical elements in the hard parts of a phthisical lung which are not to be found in the hard parts of chronically irritated tissue in any other organ. Finally, there are no elements in the induration of phthisis which are not to be found in chronic bronchitis. It is even possible that there are no anatomical elements in the induration of phthisis which are not to be found in the healthy lung. We cannot say this positively as regards man, but it is certainly so as regards the guinea-pig."[a]

Should further observations confirm the opinions which we have thus briefly sketched, the modifications which would necessarily follow in our management of consumptive patients, would neither be few nor inconsiderable. We should recommend that the bed of a

[a] Edinburgh Med. Jour., Novr. 1869, p. 393.

phthisical person should not be shared by any other individual, we should regard hemoptysis with less apprehension as an evidence of phthisis, but with far more apprehension as a cause of phthisis, and accordingly we should direct our efforts to promote the expectoration of the effused blood, we should look upon a common bronchial catarrh, arising in a delicate person as an ailment which may be followed by very serious consequences, we should be much more solicitous than we at present are, to guard the lung from the irritating effects of exposure or of changes of temperature. We should endeavour to hinder the development of the bronchial catarrh which may be followed by so formidable results, and we should be specially solicitous about the prevention of the supervention of lobular pneumonia on these apparently trifling ailments. Even in the more advanced stages of the disease, we should be less impressed with the conviction of the fatality of consumption than we now are. At present our belief in the essentially constitutional nature of consumption leads to a treatment based on this idea, to a strict attention to the general condition, and to a comparative neglect of the merely local manifestations. The hopeless view of the disease expressed by Laennec, when he said that "almost all professional men are now of the opinion that the tuberculous affection is, like the cancerous, absolutely incurable, because nature makes no efforts, but what are opposed to cure, and art can only make useless ones;" has long been regarded by physicians as too absolute. The result of recent researches will be to encourage the adoption of a still more hopeful view, and to lead to renewed efforts in the direction of the therapeutics of the malady. Certainly enough has been shown to render it in every way desirable that further investigations should be made to endeavour to arrive at definite conclusions regarding the nature of this terrible malady, and to establish its treatment on a solid and rational basis.

TREATMENT OF ANEURISM.

Dr. George W. Balfour[a] contributes a new series of cases of internal aneurism, treated by iodide of potassium, in doses of 20 or 30 grains, three times daily. His increased experience has, he believes, fully justified the opinion formerly expressed by him regarding the efficacy of this mode of treatment, as being the best hitherto devised for the relief, if not cure, of this formidable malady.

[a] Edinburgh Medical Journal, 1869.

Dr. Von Langenbeck[a] has employed the subcutaneous injection of ergotine, in the treatment of aneurism, in two cases. In one, an aneurism of the radial artery, a single injection was sufficient to cure the disease: in the other a subclavian aneurism, the remedy was used with much benefit. He believes the ergotine to act, by exciting contraction of the involuntary muscular fibres in the arterial wall.

Professor Zdekaner[b] speaks highly of the treatment of aneurism of the aorta, by the galvanic method. He introduces three to six steel needles at points about an inch and a quarter distant from each other. They are to be inserted in a convergent direction; but so that the points do not to touch. He employs moderate currents, and connects the needles with the positive pole of the battery; the negative pole being connected with a sponge, and applied at different parts of the tumour.

Hoegh[c] relates a case in which an aneurism of the arch of the aorta, was cured by the administration of large doses of acetate of lead. Four grains were given morning and evening, and the dose was increased by two grains every eighth day. When the dose reached 30 grains, symptoms of lead poisoning appeared. which yielded to treatment within a month. By this time the pulsation in the tumour had diminished. Some months after, the tumour having again begun to increase, the lead treatment was again resorted to with benefit. Five separate courses of the acetate were given during four years; the administration of the remedy being pushed to the point of the production of symptoms of lead poisoning, with the ultimate result of cure of the disease.

TREATMENT OF DIABETES MELLITUS.

In 1865 Saikowsky[d] found that the administration of arsenic to animals, had the effect of rendering the liver free from the sugar-forming or glyco-genic substance. The effect of the administration of arsenic in two cases of diabetes, has been recorded by Dr. Leube[d] the notion of the possible utility of this drug, in such cases, having been suggested to him by these experiments. The results were satisfactory in both cases; the amount of sugar in the urine diminishing notably, without any change in the diet of the patient. The first indication of improvement was a diminution in the amount of the urine, not in the proportion of sugar contained it. The remedy was

[a] Berlin Klin. Woch., 1869. No. 12.

[b] St. Petersburgh Med. Zeitschrift, 1870. 1 Hft. Prager Viertel-Jahrschrift, 1870, s.23.

[c] Norsk Magaz. for Laegevidensk, Bd. 22, Hft. 6. Virchows Jahresbericht, 1869.

[d] Deutches Archiv fur Klinische Medicin. Band 5, S 572.

prescribed in the form of Fowler's solution; and in one case $\frac{1}{3}$ grain of arsenic was taken daily for two months without inconvenience.

Dr. Owen Rees[a] had, in 1864, employed a solution of chloride of arsenic, with excellent effect in a case of diabetes, in which other remedies had failed.

Dr. Jaccoud[b] has found, that arsenic with a suitable dietary, has been sufficient to cure several cases of diabetes, in which the patients had not as yet lost flesh. He has not tried it in cases, in which much emaciation existed. He has also found the administration of strychnia, to be followed by excellent results. He uses the sulphate of strychnia dissolved in water, with the addition of syrup of orange peel; the dose at the beginning being $\frac{1}{15}$ of a grain, which is to be increased to four or five times this amount.

In a very interesting series of communications, Dr. Arthur Scott Donkin,[c] Lecturer on Forensic Medicine to the University of Durham; advocates the employment of purely milk diet in diabetes mellitus, Brights disease, disease of the supra renal capsules, and other diseases accompanied by imperfect nutrition. He records two cases of diabetes, in which the milk diet was methodically persevered in. In the one, the patient was rapidly restored to a condition of health and strength; only a little sugar remaining in the urine, to show that the disease was not completely eradicated. In the other however, the cure was rapid and complete. In the former case four grain doses of opium were given at bed-time; and one grain of sulphate of iron, and three grains of sulphate of quinine, in mixture, three times daily. In the latter, one grain of sulphate of iron, and two grains of sulphate of quinine, were given three times daily, This with six pints of skimmed milk, warmed, and divided into four meals, with an interval of four hours between each, constituted the entire dietary and treatment of the case from the beginning.

With regard to the rationale of the treatment, Dr. Donkin observes as follows:—

"Dr. Francis Home first indicated the value of an animal diet in the treatment of diabetes mellitus, and it is now nearly three-quarters of a century since the diet he indicated was reduced to a system by Dr. Rollo, in the important work he published on the subject. During the period which has since elapsed a diet more or less purely animal, as indicated

[a] Lancet: October, 1864.

[b] Nouveau Dictionnaire de Medecine et de Chirurgie. Tome 11, 1860.

[c] Lancet, November 27, 1869, p. 736.

by him, has been recognized as the proper and only available treatment to be employed against this most formidable malady. But the treatment of Rollo, either absolutely followed out or modified by the admission of certain kinds of vegetable food, is not by any means so universally successful as could be wished. The life of the patient is sometimes cut short during the treatment by the sudden accession of inflammatory affections of the abdominal and thoracic viscera. Dr. Bardsley specially directed attention to this fact. Besides, it is exceedingly difficult to enforce a strict adherence to animal food for any length of time; with some individuals, indeed, it becomes almost intolerable. Dr. Rollo himself even lamented that this mode of treatment should be so repulsive to the patient. A purely milk diet, however, is free from these objections; milk being generally relished by the patients, especially at the outset of the treatment, when the thirst is intense. Afterwards they never tire of it, although they generally express a desire for solid food in addition.

"From the experience I have already had, I have not the slightest hesitation in asserting that the milk treatment of diabetes is as superior to Rollo's, with its more modern modifications, as Rollo's was to any method previously in use. But it is necessary to add that *it must be persevered in methodically and exclusively until convalescence is established.* If this rule is not followed from the beginning the result will not be successful.

"The efficacy of this treatment need excite no surprise when we know that the disease has hitherto been amenable only to the protracted employment of a dietetic course. The *rapidity* with which milk acts is truly surprising, twenty-four hours being sufficient for the production of a marked improvement: the quantity and density of the urine suddenly fall, and, *pari passu* with this change, the thirst and voracious appetite disappear, the skin becomes moist and perspiring, while the symptoms referable to the nervous system are as rapidly relieved; profound refreshing sleep succeeds to the previously sleepless, restless condition, rendered almost intolerable by the incessant thirst. The patient expresses himself thankful for the sudden and unexpected change. In the two cases I have recorded this sudden relief was obtained. I may mention, too, that a third case is now under my care, in which by the same milk diet, and *without any other remedy whatever*, the urine fell from 23 pints, sp. gr. 1038, to 9 pints, sp. gr. 1040, in twenty-four hours, and to 6 pints, sp. gr. 1038, at the end of the third day of treatment; in other words, there was *a diminution of* 14 *pints of urine and about* 19 *oz. of sugar in twenty-four hours, and of* 17 *pints and about* 24 *oz. of sugar in three days.* A result like this seems almost incredible; but I may add that this astonishingly rapid improvement in the urine was accompanied by a corresponding rapid abatement of the other prominent symptoms of the disease. I shall, however, give a complete history of this in a subsequent communication.

I have no doubt that others will, on trial, find the remedy to act with the same magical rapidity as I have done."

Dr. George W. Balfour[a] also has found a dietary consisting exclusively of milk, as employed by Dr. Donkin, of great service in a case of diabetes. The patient, a woman, was put on six pints of milk for her sole diet, no other food whatever being taken, with the effect of reducing the amount of urine one-half, and diminishing, also, the specific gravity. This mode of treatment being simple and inexpensive, can, as he suggests, be readily carried out at home, and need not exclude the employment of such drugs as may be considered advisable.

Dr. Pavy[b] states that of all the agents which he has hitherto employed in the treatment of diabetes, opium, morphia, and codeine, have the most powerful influence in controlling the disease. Of the three remedies named, he believes codeine to have the best effect. In some cases, it has exercised a directly curative action; in others, it has mitigated the severity of the malady. The following observations on codeine, will be found to possess interest:—

"Of opium, morphia, and codeine, the last, I consider, possesses the most advantageous properties. As far as my experience, including that amongst hospital in-patients and out-patients and in private practice, enables me to judge, I cannot help thinking that codeine will be found to prove a valuable agent in the treatment of diabetes. In some cases it appears to possess the power of exerting a direct curative action. In others, however, although it has been found, particularly for a while, to control the elimination of sugar and improve the general condition, yet it must unhappily be said that it has afterwards proved powerless in averting an unfavourable termination.

"The great advantage of codeine over opium and morphia is that it is equally efficacious in controlling the disease, and does not exert the same narcotic effect. When given in a small dose to begin with, and increased gradually, nothing may be perceived beyond its effect upon the disease. Half a grain I consider an appropriate dose to begin with, and, as seen by the following cases, I have given it to the extent of ten grains three times daily. Beginning with one grain three times a day, a little drowsiness, giddiness, and perhaps headache, may be complained of. I have begun with two grains three times a day; but the dose had to be reduced to one grain on account of the disturbance of the cerebral functions that was produced. Under its employment I have not observed the pupils to

[a] Edinburgh Medical Journal, February, 1870.
[b] Guy's Hospital Reports, 1870.

become contracted, nor have I noticed any marked constipating action. It may, perhaps, have a little tendency this way, but nothing certainly to any marked extent. The tongue, in some of the cases, I have particularly noticed to be conspicuously clean and moist. The appetite, as a rule, has not been affected by it."

ON SYNCOPE.

Professor Brown-Sequard[a] announces that three varieties of syncope exist. Of these, one is familiarly known, namely that caused by cessation of the hearts action.

The second variety is less known, it consists in the stoppage of the respiratory movements, without asphyxia When the respiratory movements stop suddenly in consequence of irritation of the medulla, or of the neighbouring parts of the nervous centres; the heart continues, in the majority of cases, to beat with more or less rapidity and force. The stoppage of the respiration is not due to spasm of the diaphragm, or of the intercostal muscles, but to a cessation of action in them, brought about by a cessation of action in the respiratory nervous centre.

The third variety of syncope has not been before observed, and the author merely directs attention to it, promising to explain it more fully at a future period. In making experiments on the cause of the absence of agony in sudden death produced by irritation of the vagus, of the sympathetic nerve in the abdomen, of the spinal cord, or of the encephalon, Dr. Brown-Sequard had ascertained that the venous blood was less deep in colour than usual, and that it was sometimes as red as the arterial. Besides this, he observed a rapid lowering of temperature; and from these two facts, combined, he has come to the conclusion that certain of the interchanges which normally take place between the blood and the tissues, are either much diminished or altogether arrested.

It is remarkable that in animals in whom these phenomena have been produced, the vital properties of the nervous and contractile tissues persist much longer than when death has been caused by asphyxia, or by sudden hemorrhages.

GLOSSO-LARYNGEAL PARALYSIS.

Dr. Wilks[b] gives an account of four cases of this affection, in one of which, a post mortem examination was made by Dr. Moxon.

[a] Archives de Physiologie Novembre et Decembre, 1869.
[b] Guy's Hospital Reports, 1870, p. 1.

In this case, which was complicated by the co-existence of progressive muscular atrophy, the characteristic symptoms of this form of paralysis were present. The orbicularis oris was perfectly paralyzed, so that the saliva escaped from the mouth; the buccinators were paralyzed: the patient had lost power over the tongue, and his speech was scarcely intelligible. There were other symptoms present referable to the progressive muscular atrophy. The post mortem showed atrophy of the roots of the hypoglossal, of the inner roots of the spinal accessory, and of the whole of the roots of the anterior spinal nerves. The anterior half of the spinal cord showed atrophy, and hardness of the white substance, the grey substance being enlarged and reddened.

There was a red-grey change visible in the floor of the fourth ventricle, involving the nib of the calamus scriptorius, and extending upwards and outwards for half an inch.

Dr. Wilks draws attention to the dissections of Mr. Lockhart Clarke, which demonstrate the connexion between nuclei of the facial, vagus, hypoglossal, and laryngeal nerves, and offers the following ingenious suggestions:—

"It appears remarkable that a small area in the medulla oblongata, or even the olivary body, coinciding with a physiological centre as that of articulation, should be picked out to undergo a rapid or slow morbid change. So remarkable is the fact that it might be worthy, in the first place, of inquiry whether or not experience justifies us in declaring that definite parts of the cerebro-spinal centres having special functions are more prone to disease than other portions of the brain and spinal cord taken indifferently; whether, indeed, all parts are not equally liable to inflammation and degeneration, but it is only when certain physiological portions are affected that we are enabled to apply our names, because then the seat of disease has made itself manifest by the implication of nerves whose function is known, whilst in other cases we are content to use such expressions as cerebral or spinal disease. Although I believe this to be to a certain extent true, yet I consider it is proved that those parts of the cerebro-spinal system which have definite physiological properties are more liable to disease than other spots taken indiscriminately; if so it may show, as is most probably the case, that the vascular supply of such physiological centres is accurately defined and circumscribed, or it may be that a centre having a definite function, being the focus of a number of nerve filaments proceeding from it for a special purpose, must soon be affected if any of these filaments be primarily attacked, seeing that morbid processes quickly proceed along given anatomical tracks. If, then, degenerative processes occur in connexion with a morbid state of blood-

vessels, and if the anatomical supply bears a relation to defined physiological areas, the explanation of such parts being selected for chronic disease is not so difficult; and if again morbid processes proceed rapidly along nerve filaments, we can understand also how parts having intimate relations are concurrently affected. It is remarkable circumstance that there is no disease of the nervous system, as far as I am aware, which may prove fatal, and even show a well-marked lesion or degenerative change after death, but may have its counterpart in a functional and curable disorder. This must clearly show that, although in actual blood effusions or softenings the seat of disease may appear accidental, these centres must have been affected from a more precise anatomical reason. For example, an effusion of blood in the corpus striatum productive of a hemiplegia may appear to be accidental, but a temporary hemiplegia of a few hours' duration, as seen after an epileptic fit, and due to a derangement of a part having a very limited area, can only be accounted for on the supposition that physiological districts of the brain are anatomically defined. It is the same with diseases of the spinal cord, every variety of which may occur as a temporary disorder. Let it be said, for example, that paralysis of motion and sensation imply definite lesions of the motor and sensory tracts, then the function of these tracts must be in temporary abeyance to account for the paralysis which occurs in those patients who perfectly recover. Again, let it be said that distinct nervous lesions are discovered in cases of progressive muscular atrophy; we must suppose that in those very severe cases which recover these same special parts of the cord were for a time powerless. Or, if in locomotor ataxy it be shown that the posterior columns of the cord have undergone grey degeneration, it must be believed that those same parts are involved in some way in the cases which recover. All these circumstances would show that of the various nervous ailments which we meet with a large number have a determinate and fixed character owing to certain distinct anatomical and physiological centres being involved. In hysteria it is known that every possible nervous disorder may be simulated, and amongst these I have seen a tolerably fair example of labio-glosso-laryngeal paralysis."

ACTION OF DIGITALIS ON THE HEART.

Dr. Paul Lorain[a] furnishes an interesting contribution to our knowledge of the effect of digitalis on the heart, which he has studied by aid of the sphygmograph.

The conclusions at which he has arrived are—

[a] Journal de L'Anatomie, &c., Mars et Avril, 1870.

1st.—That in moderate doses, digitalis restores the force of the heart, and renders its action slow and regular.

2nd.—That administered in excessive quantity, or for too long a time, it deranges the action of the heart, and causes irregularity of the pulse.

In the case of a man of seventy years, suffering from mitral incompetence and anasarca, under the influence of digitalis in powder, to the amount of $2\frac{1}{2}$ grains daily, the pulse which had been quick, weak, and irregular, became stronger, more frequent, and perfectly regular. When the remedy was continued too long, or the dose unduly increased, the pulse became very slow, falling to 32 in the minute. Dr. Lorain has observed that in cases in which the pulse has become slow under the influence of this drug, the cardiac contractions are more numerous than the beats of the pulse which are to be felt in the wrist. When the pulse was 32, the stethoscope enabled him to recognize that there were 64 systoles of the heart, that is exactly two ventricular contractions for each beat of the pulse. Of these, one, that which corresponded to the pulse was strong; the other was very weak, but distinctly audible; and its effect, although too slight to be perceptible by the finger, was to be recognized in the sphygmographic tracing.

PUNCTURE OF THE ABDOMEN IN TYMPANITES.

Dr. Julius Stein[a] details two cases in which the abdomen was punctured with a fine trocar for the relief of gaseous distension, with excellent effect.

In one patient in whom enormous distension of the abdomen was produced by the pressure of an ovarian tumour on the intestine; the puncture was made in the cecal region, and was repeated daily, at the urgent request of the patient; over fifty punctures having been made during the last period of the disease. At the post mortem hardly a trace of these numerous punctures could be observed, and there was absolutely no evidence of any irritation of the intestine having been produced by them.

In the second case, that of a man of sixty-one years, eight successive punctures were made during fourteen days. The trocar was inserted, in this case, below the ensiform cartillage, and, as in

[a] Deutches Archiv. fur Klinische, Medicin. Band, VI., heft 4. 1869.

the previous case, the operation gave great relief to the patient; and was followed by no unpleasant sequelæ.

Dr. T. Clifford Allbutt[a] details the following interesting case:—

"——, aged 40, labourer, was admitted into the Leeds Infirmary on January the 4th. He was admitted as suffering from 'obstruction of the bowels.' On investigation, Mr. Bradley found that although the bowel symptoms were more prominent, yet the patient was suffering also from double pneumonia. He told me that he was led to examine the lungs because he had seen obstruction of the bowels more than once in connexion with pneumonia. I was also aware of this co-existence of paralysed bowel with pneumonia, and have explained it by supposing that irritation from the inflamed structures reaches, and finally exhausts, the colon. I have certainly seen it in several well-marked cases, when it has proved a very distressing and unmanageable symptom. My explanation seemed certainly to be true in a somewhat similar case of bowel paralysis attended by Sir William Jenner, Dr. Beaumont of Knaresborough, Mr. Bainbridge of Harrogate, and myself,[b] where paralysis of the bowel was set up by inflammation near it, and such paralysis, we know, often occurs in peritonitis. In such cases of simple paralysis, or of fecal accumulation with distension, I think the mode of treatment I am about to propose may turn out to be of great importance. In the present case Mr. Bradley administered subcutaneous injections of morphia, and full enemata both with and without turpentine and castor-oil. No relief was obtained. No feces had now passsed from the bowel for at least five days, and no wind had passed for about two days. The abdomen was enormously distended, and caused intense suffering; the pulse was small and quick, the extremities cold, and the countenance very anxious. In the evening Mr. Bradley requested me to see the patient. His state remained the same. I advised an injection of a quart or more of warm gruel to be slowly administered with a long tube, this to be followed by an injection of half a pint of warm olive-oil in the hope that the oil might rise through the gruel and reach the obstruction. Belladonna liniment was also to be applied all over the abdomen, and the subcutaneous injections to be continued. About three pints of the fluid were got into the bowel, but no feculent discharge followed: the liniment gave some relief. Next morning I found the patient in great suffering; he had not passed any wind, and he had spent a wretched night. He now had also a decided hiccough, and the abdomen was enormously distended. There neither was, nor had been, much vomiting.

[a] The Practitioner, February, 1869, p. 109.

[b] Vide Lancet, July 18, 1868.

The previous evening I had been much tempted to puncture the abdomen with a fine trochar, and in the morning the desire strongly returned. Somewhat later in the day I was with Mr. Teale, and after laying the case before him, I asked him if he would operate for me. He agreed to do so, and we visited the infirmary at once. We found the patient in great distress; he was tossing restlessly about in bed, complaining of the distension of his abdomen, and the breathing was seriously impeded. Mr. Teale plunged an exploring trochar (No. 1. Weiss) into the distended transverse colon. On withdrawing the trochar air rushed through the canula with a hissing noise and the abdomen slowly fell. The air was highly offensive. When the current grew weaker Mr. Teale made a second puncture into the descending colon, which was greatly swollen: the result was the same. The body had now fallen much, and had become quite soft; the patient expressed the greatest relief, and thanked us warmly. His countenance became placid, the breathing comparatively easy, and he soon sank into sleep. I ordered the morphia to be continued, and poultices to be kept on the body. On my visit next day I found our treatment had been remarkably successful. Not only had the relief continued, and the poor man slept comfortably, but the bowels had recovered their activity. He had passed a great quantity of wind to his great comfort, and many stools; some of these stools were seen by Mr. Bradley, who reports that they were copious and feculent. So far, then the operation seeemed to have been eminently successful. Unfortunately the pneumonia had not stayed its course, and the whole of one lung behind was now consolidated, and more than half of the other. He sank from the lung mischief in about two days more—the abdomen becoming again somewhat distended before death. At the autopsy, almost the whole of the left lung was found in a state of 'hepatization, and a large part of the right lung also. The other organs were healthy; in particular there was no trace of peritonitis, nor had any air escaped into the peritoneal cavity. No traces of the punctures could be detected except upon the outside of the body. I need not stay to point out how often patients beg for relief from a distended abdomen, nor, I think, to show that the simple and painless operation which we practised proved to be quite without danger, and gave great and immediate relief, not only by emptying the bowel of wind, but by enabling it to contract upon, and to expel, the feces which obstructed it. The operation is one which any physician may have recourse to in a moment, and if he has not a fine trochar at hand he may use the needle of his morphia syringe. I may add, that the autopsy did not seem to support my view of the causation of the bowel paralysis. There was no appearance of inflammation on the under side of the diaphragm, nor was the surface of the bowel injected with blood.'"

DIAGNOSIS OF RENAL DISEASE FROM BLOOD-CORPUSCLES IN THE URINE.

Dr. Joseph G. Richardson,[a] Microscopist, to the Pennsylvania Hospital, Philadelphia, has applied to the elucidation of the condition of the kidney in renal disease, the well-known views of Cohnheim, regarding the relation of the white blood-corpuscles to the inflammatory process. If both red and white corpuscles are present in the urine, and if it can be ascertained that their occurrence is independent of vesical irritation, the following indication may be derived from their relative proportion.

When the red corpuscles greatly exceed the white in number, true hemorrhage has taken place from the kidney. This conclusion will be strengthened if no tube-casts be found, and the albumen be present in such quantity as will correspond to the amount of corpuscles, supposing both to be furnished by the liquor sanguinis.

When the white corpuscles are more than $\frac{1}{2\,5}$th of the red, there is inflammation of the kidney of either an acute or a sub-acute kind, and the danger will be found to be in direct proportion to the total amount of the corpuscles.

When few or no red corpuscles are found to be mixed with the white chronic inflammation is present, and is proportionate in extent to the number of blood-corpuscles.

In cases of acute Bright's disease, passing into the stage of fatty degeneration, the appearance in the urine of epithelium, containing oil globules points to the change from the stage of inflammation, and these epithelial cells will be found, according to the author, to vary inversely with the number of white corpuscles present, the albumen remaining the same in amount.

[a] American Journal of the Medical Sciences, January, 1870.

REPORT ON OBSTETRIC MEDICINE AND SURGERY.[a]

By George H. Kidd, F.R.C.S.I., L.K.&Q.C.P.; Obstetric Surgeon to the Coombe Lying-in Hospital; Vice-Pres. Obstet. Soc., Ireland; Cor. Mem. of the Gynecological Soc. of Boston.

STATE OF THE PULSE IMMEDIATELY BEFORE AND IMMEDIATELY AFTER PARTURITION.

The pulse during the puerperal state was made the subject of a series of observations by Dr. Hémey[b] in 1864, while serving as interne under Dr. Empis, and he believes his observations established two remarkable phenomena:—First, the lowering of the pulse (already noted and studied by M. Blot), with regard to which Dr. Hémey says his observations confirm those of Blot. Second, the inequality and irregularity of the pulse, which had not hitherto attracted attention.

Dr. Hémey's observations seem to have been made with very great care, and are recorded with great minuteness of detail, and therefore seem worthy of having an abstract of them recorded here; but the results arrived at are different from those of Dr. M'Clintock, published in this Journal in May, 1861; and also from those arrived at from a series of observations made by the writer of this report in the Coombe Lying-in Hospital.

Dr. Hémey takes 75 as the average pulse of a healthy woman. Before labour it is from 75 to 84; but he does not count any pulse as lowered, unless it is 60 or under, and by adopting this as his standard, he believes he excludes all doubtful cases from his tables. He thinks at least one-sixth of all newly confined women have their pulse lowered. In one case the pulse was as low as 44; in many he found it 48; and in a great number 52. The lowering shows itself most frequently some hours after labour, and continues sometimes without variation; in some it increases during the eight days. At the end of the third, fourth, or fifth day the pulse sometimes becomes relatively more frequent and falls again, or it may preserve this frequency to the end; but these changes seem to depend on accidental circumstances, and independently of such the lowering appears to have its maximum at the end of the second day from labour, and to diminish gradually and cease about the tenth day.

[a] The author of this Report, anxious that every contribution to obstetric medicine and surgery should be noticed, will be glad to receive publications on the subject. They may be sent to the publishers of the Journal through their correspondents.

[b] Recherches sur le Pouls Pendant les Quinze Jours qui Précèdent on qui Suivent l'Accouchement. Par le Dr. L. Hémey. Archives Générales de Médecine, Vol. II p. 154.

Out of 100 consecutive cases in which the daily state of the pulse was recorded in the Coombe Lying-in Hospital, it was found as low as 60 in five cases and within the first 24 hours after delivery; and in each it had risen to from 70 to 84 on the second day, and maintained this average as long as the patient was in the hospital. In one case the pulse was as low as 56 eight hours after labour. This woman had been delivered in the street, on her way to the hospital. On the second day the pulse was 72, at which it remained till the fourth day, when it fell to 65. On the fifth day it was 72; and on the seventh, which is the last observation recorded, it was 84. This was the only case of the series in which the pulse was under 60.

The duration of labour, unless it be prolonged so as to do local or general injury, does not seem to influence the changes the pulse undergoes, but the occurrence of gastric derangement with headache, of a foul tongue with constipation and hot skin, on the second day may raise a pulse to 80 or 88, which the evening before was only 56, and Dr. Hémey observes, as has already been remarked by M. Blot, this increase of 30 is of more importance than would attach to the total number of 80, and the increase is soon proved to belong to a pathological condition by the action of a purgative which reduces the pulse again to 54 or 60. When there is metritis or sloughing of the vagina, or puerperal fever, the characteristic changes on the pulse are more permanent.

Dr. Hémey combined with his researches on the pulse some observations as to the presence of sugar in the urine of women lately confined. M. Blot announced its existence, in a paper read at the Academy in 1856, and M. Lecomte denied it in a communication read in the following June; but in 1859, Professor Bruecke confirmed the statements of M. Blot. Dr. Hémey found that of one hundred newly confined women, twenty-seven had a quantity of sugar, varying from eight to eighteen grammes per litre in their urine; the urine of thirty-five presented traces of it only; and in thirty-eight no appreciable trace of it could be discovered. Its presence was of short duration; even when most abundant, it could be found for a day or two only, and it did not seem to be in any way connected with the secretion of milk, and it did not seem to influence the condition of the pulse, for though in fifteen or twenty-seven cases where it abounded, the pulse was quick; in twelve other cases it was remarkably slow.

With regard to the influence of the secretion of milk, Dr. Hémey found that from the second to the fifth day, the pulse did not

undergo any notable change in two hundred and eighty women out of four hundred; he believes, however, that in many cases where the pulse did not seem to undergo any change from the second to the fourth day, it was because the lowering of it was interfered with by the state of the breasts, and he finds the best nurses, and those who have the most abundant supply of milk, are those in whom the pulse is lowest and that without interruption from the second to the tenth day.

The period of pregnancy at which labour occurs does not seem to influence the changes undergone by the pulse in any way. In one case when it occurred at the end of the fifth month in consequence of a fall, the pulse, which before labour was 80, fell to 76, 68, and 52, on the following days, and then rose again to 72; but in other cases of premature labour no lowering of the pulse was observed. Where the fetus is dead for some time before labour sets in, the lowering of the pulse seems to begin at the time of the child's death, rather than when it is expelled, and Dr. Hémey gives a series of cases illustrative of this as the fact is made use of in progress of the essay.

After-pains may not only, as M. Blot has already remarked, coincide with a slow pulse; but, according to Dr. Hémey's observations, when they occur independently of any other pathological condition, they are rarely accompanied by a quick pulse. Moral emotions, however, have a great influence in raising the pulse; diet does not affect it unless the process of digestion be deranged, and then the pulse is more easily affected in the puerperal state than it would be at other times. Change of position will also affect it more notably at this time, so that a woman lying down may have her pulse only 68, and it may rise to 104 when she sits up. Primiparæ seem less disposed to have the pulse lowered than multiparaæ, perhaps because the former are more subject to the multitude of little causes which tend to accelerate it.

As a prognostic sign, Dr. Hémey looks on this lowering of the pulse as most favourable. It occurs most frequently when the health of puerperal women is best, and it is oftenest met with among the vigorous and those who have the most abundant supply of milk; and he has never known a woman who presented it to be affected with puerperal symptoms of any gravity.

As to the cause of this lowering of the pulse, Dr. Hémey argues that it depends on an increased arterial tension caused by the rapid withdrawal from the general circulation of a certain number of arterial trunks; that is to say an increased arterial tension caused by

the large quantity of blood hitherto required by the uterus being thrown into the general circulation. It may be regarded as proved, he says, and it has been demonstrated by the sphygmograph, that the frequency of the pulse is in the inverse ratio to the arterial tension; and it can also be demonstrated by the same instrument, that the arterial tension is increased by compressing one or more arterial trunks, so as to withdraw them from the general circulation. Marey and Blot, he says, assign the same cause; but he finds new proofs of its being the cause in the facts related in reference to premature confinements; to the cases where the child died before birth; and in the effect of the seasons. In premature confinement the same cause for the increased arterial tension exists as when birth takes place at the full term, and when the child dies before birth it is then the supply of blood to the uterus is diminished, and consequently it is then the pulse is lowered, and not after the child is expelled; and it is well known that cold increases the arterial tension, and the lowering of the pulse is most marked in the cold seasons.

The analysis of four hundred observations gives ninety-four cases in which the rhythm of the pulse was altered; most frequently it was irregular and uneven at the same time; exceptionally it was irregular or unequal only. Dr. Hémey has met with these phenomena but twice before labour, and in both cases the patient had old organic disease of the heart. In all the other cases the irregularity showed itself from the first to the tenth day after labour, with the exception of three in which it did not appear until the fifteenth or eighteenth day; but it is to be noted that very few of the observations extended beyond the twelfth day, so that the extreme limit of the occurrence has not been fixed. In some cases the irregularity disappears quickly, but may return soon, and it is rare to find it persist more than six days without interruption. The irregularity and inequality may accompany one another, or exist separately, and these modifications always supervene very rapidly on one another. Of the ninety-four cases, the irregularity without inequality was only observed in twelve, and the inequality without irregularity in eight; very often the irregularity and inequality exist with the slowness of the pulse, but the slowness may be found without the others, and they may occur with slight acceleration of the pulse.

The prolongation of labour does not of itself seem to influence these phenomena, but when it causes any injurious consequences the irregularity disappears. It is in fact rare to find a pulse febrile and irregular. There does not seem to be any relation between

these phenomena and the secretion of milk, except that milk fever, like all other febrile conditions, will cause them to disappear; neither does the term of pregnancy, nor the state of the life or death of the child seem to produce any effect; nor do moral emotions, except so far as by quickening the pulse they cause irregularities to disappear. Pursuing the investigation under the same heads as in reference to the slowness of the pulse, Dr. Hémey does not find that the diet or age of the patient, primiparity or multiparity, nor yet the season of the year exercises any influence; nor does he think the presence of these phenomena indicates anything unfavourable as to the recovery of the patient; and he is unable to suggest any cause for them beyond some obscure condition of the nervous system.

PUERPERAL TEMPERATURES.

It is to be regretted that this subject has not received the attention it deserves, but the following extract from Mr. Squire's paper in Vol. ix. of *Obstetric Transactions* are of interest, especially in connexion with those of Dr. Hémey on the pulse. The observations were made chiefly in the axilla.

"The temperature of the body is somewhat increased during pregnancy, at least in the latter months. The normal temperature may be taken at 98°·75, the temperature after the sixth month of pregnancy will be frequently found somewhat over 99°. A variation is to be observed in different persons, and in the same person in different states of health; the limits of this variation, as consistent with health having been restricted in the cases I have noted, to one degree, viz., from 98°·8 to 99°·8, its range being less than in women of similar age when not pregnant. Some remarkable oscillations of temperature, such as we need not expect to find in pregnancy, have occurred in a few cases that have presented themselves to me for notice, which tend to show that there is a considerable fall in temperature on the occurrence of the catamenia, and a variable rise shortly before: in one instance the vaginal temperature was found to be 99°·8 on the day before the catamenial flow, and only 97°·3 on the day after. No observation was obtained on the intermediate day; the flow was not very free, and was unaided by alcoholic stimulants, indeed, these had been abstained from for nearly a month, until this day, when they were resumed; three days afterwards the temperature was 98°·7, the period being then over: it had been in every respect normal, yet there was a fall of 2½ degrees in the first two days of its occurrence. In another case, where a vaginal operation was performed on the day preceding the expected period (which recurred with perfect regularity), the local temperature was 100°·1;

eight days afterwards, though there was some persistent local irritation, the thermometer did not go beyond 98°·7. There may be considerable pain, tenderness, and sensation of heat, without the thermometer being much affected; in a case where these conditions were all present from the too free application of undiluted liquor plumbi to verrucæ of the labia minora and fourchette, the local temperature was only 98°·9, and even this was accompanied by a slight increase in the general temperature as taken in the axilla.

"The difference between the vaginal and axillary temperatures will seldom be more than one-third of a degree, and frequently only one-fifth or one-tenth, if in the latter situation all the requisite precautions to secure accuracy are observed. In this way 98°·45 was obtained as the normal temperature in the axilla, and 98°·75 in the vagina, where there was no disturbing circumstances; in a case of abortion at two months, the vaginal temperature on the ninth day was 98°·4, the axillary temperature 98°.

"A comparison of these differences was made in the puerperal state; first, on the second day of delivery where slight perineal fissure existed, the temperature there was 98°·5, in the axilla 98°·3; so that a local injury had not disturbed the usual relative temperatures: second, on the fifth day of delivery, where the breasts were very full and tender, the milk increasing and the lochia decreasing, the axillary temperature had risen to 100°·3, the vaginal temperature was 100°·7; so that while in full activity the mammary gland does not, by its proximity to the axilla, unduly influence the results there obtained by the thermometer: in the third case, on the ninth day, a final application was required to complete the cicatrization of a perineal fissure, the temperature there was 98°·3, that at the axilla 98°·2; in this latter situation there was probably now less difficulty in obtaining the true temperature, on account of the process of lactation being in full activity.

"The commotion and efforts of parturition itself, while confined within the ordinary and safe limits of natural labour, cause but a slight elevation of temperature. In the twelve cases, of which tabulated diagrams accompany this paper, this point was made the subject of exact observation in cases 1 and 9; it had previously attracted my attention in a case of prolonged first labour, which came under my notice last autumn, where there was a great heat of surface, thirst, and remarkable dryness and heat of the passages, but no corresponding effect upon the thermometer; the patient had passed a night without sleep, and the greater part of the day without food: egg and milk, and a liberal supply of beef-tea, enabled the second stage of labour to be completed favourably. In case No. 1, on the diagram, the labour was more rapid, but of some severity, with considerable agitation, heat, and perspiration, and the development of the passages proceeding rapidly; the axillary temperature could not be obtained at even the usual standard; there was,

however, a progressive increase shown by the thermometer used in the vaginal examinations; the highest reading, obtained half an hour before delivery here, was 99°·9, in the axilla only 97°·8. In case No. 9, where the labour was more rapid, but much less severe, the temperature per vaginam, two hours after the commencement of labour, was 98°·9; in a similar observation taken, taken twenty minutes before the expulsion of the child, it was 99°·1, and ten minutes after, exactly 99°.

"The elevation of temperature thus occasioned, is maintained to the termination of labour; then commences a steady decline, which in most cases, if not all, has reached the normal line and in some has descended considerably below it. This subsidence always takes place in the first twenty-four hours; it may, however, be prolonged into the second day, as in Case 9, or be accomplished within the twelve hours, as occurred in Case 6. In this case delivery took place at 3 a.m. with some hemorrhage, sleep followed and the mammary secretion was rapidly established. In Case 9, a short labour terminated at 11 p.m., with no loss of blood, and followed by excitement rather than sleep, the lowest point was not reached until the second night, thirty hours after delivery. In Case 4, delivery at 7 a.m.; the temperature at 2 p.m. was 99°·4, she had not slept, had only taken gruel, she herself felt cool and rather faint; after sleep the temperature soon subsided to the ordinary degree. A difference is noticeable in following the line of temperature in this case, it is the only one of the series in which suckling was not attempted. In Case 1, the high temperature noted during labour had fallen four hours afterwards to 99°·2; the diagram in this case gives the simplest form of disturbance of temperature common to the puerperal state.

"In all the cases here investigated, the most constant and most obvious disturbance of temperature is that which ushers in, and accompanies, the formation of milk. The commencement of this reaction is most regular; it attains a certain prominence forty-eight hours after the birth of the child. In only one of these twelve cases was this rise of temperature retarded, in only two was it accelerated. The termination of this reaction is most variable; the modification which the line of temperature now undergoes from the merest accidents, before it settles down into the normal line at the period of convalescence, fully warrants the care and watchfulness at this time traditionally conceded.

"Where the secretion of milk is readily established, the rise in temperature necessary to its formation at once experiences a considerable fall; this is shown in Nos. 1, 3, 6, and 9, where a fall of at least one degree occurs within twenty-four hours; in the three last the temperature has in that time fallen to a point as much below the ordinary temperature as it had previously been above it. In these cases there was at the same time a very free flow of milk.

"To obviate any sources of error due to diurnal oscillations of temperature, observations were taken in Case 2 twice every day, between nine

and ten o'clock morning and evening, until the fifth day, at noon on some of these days, and in the afternoon on the subsequent days; these showed a uniform progression in the direction indicated, and it was not until the puerperal state was nearly over, and the convalescence well advanced, that the ordinary diurnal variations of temperature again became evident.

"The general conclusions offered to this Society as fairly deducible from these observations are—

"1st.—That no great elevation of temperature arises in natural labour.

"2nd.—That there is afterwards a considerable fall of temperature which is favoured by sleep.

"3rd.—That there is a subsequent exaltation of temperature which has for its natural termination the secretion of milk.

"4th.—That the bringing the method of observation here followed to the study of the puerperal state, would add an element of certainty to the principles and details of its management, and afford an additional guide for safe conduct through some of its complications."

PLACENTA PREVIA.

Dr. T. Gaillard Thomas[a] relates eight cases of this complication, and urges the rapid induction of labour.

"The dangers attendant upon the condition develop themselves most markedly in the first stage of labour, and death not unfrequently occurs before the os externum is dilated to a size not greater than a Spanish dollar. At this time surgical interference, if resorted to to accomplish delivery, often destroys the lives which it is intended to save. The hand forced too soon through a rigid os will often rupture its walls, while a delay without the adoption of the means capable of controlling hemorrhage will necessarily favour the occurrence of a fatal result.

"On the other hand, should full dilation of the os have taken place, and the patient be exhausted from sanguineous loss, the practice of rapid artificial delivery will not rarely be followed by fatal prostration.

"There is no question, in my mind, of the fact, that when it becomes the recognized practice to resort to premature delivery as a prophylactic measure in these cases, the statistics which have been quoted will be very much improved upon. By resorting to this measure we should be dealing with a woman who is not exhausted by repeated hemorrhages; the obstetrician would be in attendance at the commencement of the labour; and he would be able by hydrostatic pressure to control flooding, while the same pressure accomplished rapidly and certainly the first stage of labour."

THE RECTUM IN ITS RELATION TO UTERINE DISEASE.

Under this title Dr. Storer contributes a series of papers of very great interest, to the *American Journal of Obstetrics*. He

[a] American Journal of Obstetrics and Diseases of Women and Children, Vol. i., pp. 2, 3.

argues that the frequency of organic disease of the rectum in women is, as a general thing, under-estimated; and that the importance of such disease in exaggerating or modifying the character of uterine disease is, as a general thing, not appreciated or recognized. Dr. Storer has found the following method of examining the rectum so useful that he has long since thrown aside the anal speculum for purposes of diagnosis in women.

"By passing the finger into the vagina, and pressing backward and downward over the levator ani, the rectum can be everted through its sphincter like the finger of a glove. This can ordinarily be done to a very great degree; it can always be done to a certain extent. Should the sphincter be unusually irritable, and spasmodically contracting with violence when touched from below or thus from above, it can be forcibly distended by the thumbs, and temporarily ruptured, as I am in the habit of doing in such cases; the procedure above indicated thus becoming easy. We can in this manner ascertain the presence of chancre or chancroid, the character of polypi, the extent and number of internal hemorrhoids, the position of the inner orifice in fistula, &c., with far greater certainty and alacrity than by speculum, or can be done in the male, while the mere eversion process, provided rupture of the sphincter is not necessary, is attended by very little pain."

COCCYODYNIA.

Dr. Nott records an aggravated case of this affection cured by the extirpation of two bones of the coccyx; an operation he claims to have been the first to perform, which he has done a number of times, and always with more or less perfect relief. The subcutaneous division of the surrounding tissues, as recommended by Simpson, he has not yet tried, but he purposes to do so. He expresses his view as to the pathology of the disease in the following passage:—

"It has then been suggested that coccyodynia depends on a morbid condition of the periosteum of the coccyx, of the tendons of the muscles attached to it, of some of the small muscles themselves, of the surrounding fibrous tissues, of the nervous fibrils, and, lastly, of the *glandula coccygea;* but, so far as I am informed, we have, as yet, no reliable data for determining the pathology of this affection. If the seat is really this gland, it is a curious inquiry to ascertain whether the gland belongs alone to the female sex.

"I will venture to start another suggestion with regard to the pathology. It is a well-known law, that where any chronic irritation is brought to bear on a muscle, a strong tendency to spasmodic contraction

[a] American Journal of Obstetrics, Vol. i., No. 3, p. 243.

results, and acute pain is produced when any attempt to elongate the contracted muscle is made. Familiar examples of this are seen in the sphincters of the anus and vagina, from fissures of anus or ostium vaginæ, or other sources of irritation; vaginismus from irritation of the caruncule myrtiformes; or, as sometimes happens, vaginismus from fissure of anus, or from hemorrhoids. In all these, some point of local irritation in the vicinity of the muscles excites spasmodic contraction, and any attempt at dilatation of the circular muscle gives exquisite pain. Future and more careful investigations may possibly show that the pain in coccyodynia is attributable to the spasmodic contraction of some of the muscular fibres connected with the coccyx. There is little pain except when motion is communicated to some one or more of the muscles connected with the coccyx, and in this it differs from true neuralgia.

"Division of the muscles in question, as in fissure of the anus, and in vaginismus, gives relief."

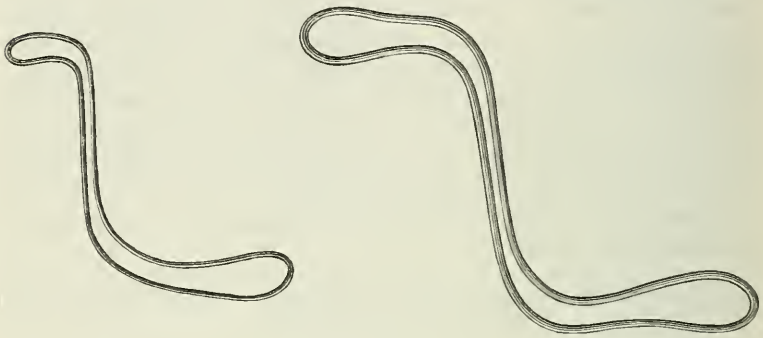
Combined Wire Speculum and Retractor. By Francis H. Brown, M.D., Boston.

"A few weeks ago, I was asked by Dr. Waterman to assist him in a vaginal operation, which required the passage to be fully shown, and indeed, was quite protracted in its performance. I took occasion to suggest employing a simple wire speculum, which would answer the additional purpose of a retractor, in place of the Sims and Boston (Storer) specula and the copper spatulæ he was proposing to use. He carried out my idea by bending a stout wire on itself, and then shaping both folds nearly into the form of the letter S; and thus got a very excellent and serviceable instrument. After I made more carefully, and, after some experiments, the two forms of the instrument which I now describe.

"They are made of iron wire, joined by hard solder and then nickelized. The wire used is numbered 10 and 13 of Stubb's (English) wire gauge. The general description of the larger instrument would be that it is a *skeleton* Sims' speculum; very nearly of the same size, and with the same curves; the two handles turning in opposite, instead of the same direction; and one blade somewhat shorter and narrower than the other. The instrument made from the smaller wire is, generally, smaller, and has one of its ends proportionately shorter; otherwise its general shape is the same.

"The objects for which I propose these instruments, are the following:—Singly, the larger instrument has been found well adapted to take the place of Sims' speculum. It displays the vagina equally well; it is much lighter; equally strong; and no less easy of application. The two together, as retractors or in place of the copper spatulæ, have served well to dilate the vagina, and bring into view every part of its wall, up to the cervix. Of course, this use pre-supposes one or more assistants;

but, when so employed and guided by the eye of competent helpers, they fill, more satisfactorily than the bivalve and multivalve specula, the place of these instruments, while they are more fully out of the way of of the operator. They have, moreover, the additional advantage over the common copper spatulæ, that they have no sharp edges.



"Singly the instrument may, not unfrequently, be used to hold and render tense the vagina, and, by its fenestrum, bring into clear view and make prominent an abscess or small tumour, an ulcer or fistula, on which one desires to operate, much as the fingers of the operator or assistant would do on the external surface of the body. It may, not inaptly, be employed after rupture of the sphincter ani for the display of and operations on internal hemorrhoids and ulcers of the rectum, or the internal opening of a fistula in ano. The retractor will be found a useful aid in searching, by means of its fenestrum, for vesico-vaginal fistulæ—which, as is known, sometimes baffle the search of the surgeon for a considerable time.

"The smaller instruments are to be used especially as retractors, to hold aside the edges of wounds during various operations for the removal of tumours, ligature of arteries, &c., and to keep out of danger arteries, veins, nerves, and similar important parts. The smaller retractor answers the purpose of the wire tongue depressor, devised by Dr. Allen, of Cambridge, and serves as an admirable retractor to draw aside the cheek or lips in various operations about the mouth and maxillary bones.

"A still farther advantage of these instruments is in their very trifling expense; the whole set of four being furnished by our instrument makers at a sixth part of the price charged for any of the elaborate forms of specula."

HALF-YEARLY REPORT ON PUBLIC HEALTH.[a]

By Charles A. Cameron, Ph.D., M.D., L.K. & Q.C.P.; Prof. of Hygiene in the Royal College of Surgeons; Lecturer on Chemistry in Steevens' Hospital Medical College, and the Ledwich School of Medicine; Analyst to the City of Dublin.

WATER IN RELATION TO THE PUBLIC HEALTH.

Within the last twenty years great attention has been given to the subject of the water supplies of towns. During the middle ages almost every European town was supplied with water derived from superficial wells situated within the urban limits. At the present time, when an elaborate system of sewerage works conveys the waste matters of towns into rivers or the ocean, the water of town wells and pumps is often, nay, generally, loaded with dangerous organic matter. We may therefore easily imagine how impure must have been the water obtained from town wells in mediæval ages, when the soil was saturated with those waste animal and vegetable matters which in modern times are happily got rid of by subterranean conduits. In former ages the mortality of towns was frightfully in excess of that of country districts. In many instances the death-rate of a crowded city was more than double that of rural districts situated under the same climatic conditions. Narrow streets, overcrowded and ill-ventilated tenements, defective provision for the removal of effete matters, and personal uncleanliness, were prime causes of the shortness of human life in mediæval times—indeed, in towns long after that gloomy period of European history had passed away. But a prime factor in the causes of the unhealthiness of towns was, we believe, the bad water which the town wells supplied. When fearful epidemics decimated the inhabitants of cities the cry often arose that the wells were poisoned. This complaint was in general only too true; but the poison in the wells was not put into them designedly—it was derived from the foul drainage of the soil, tainted with the matters thrown out from the bodies of the sick. We feel a strong conviction that in former times one of the most active agents in spreading epidemic and endemic diseases amongst crowded centres of population was impure potable water.

During recent years matters with respect to supplies of water for domestic purposes in town and country appear to be very much

[a] The author of this report will be glad to receive any books, pamphlets, or papers relating to hygiene, dietetics, &c. They may be forwarded through the agencies of this Journal.

reversed. Formerly the citizen, as a rule, was obliged to drink water of bad or doubtful quality, whilst the denizen of country parts could easily get his draughts of the pure element from crystal spring or sparkling rivulet. Now, the inhabitants of the larger towns are gradually giving up the use of pump water, and are substituting therefor pure water obtained from distant lakes and rivers. On the other hand, we fear, that owing to a variety of causes, the water used for domestic purposes is not, in general, as pure as it was in the middle ages. In the first place, the rural population has greatly increased, and there is, consequently, more effete animal matter thrown into the soil. Secondly, tillage husbandry, which includes the application of large quantities of manure to the land, has been largely extended. It may be that the potable water in use in country districts is not greatly inferior to that used in the middle ages; but that there is some deterioration in its quality is certain: and this fact is evident, that most of the large towns are now supplied with better water than is obtainable in most villages and in a large proportion of detached country-houses.

The drainage of the mere surface of the ground may be very pure or the reverse, according to the nature of the soil, the density of the population, the state of the water-shed, and the provisions for the removal of organic refuse. In every place where a dense population exists, it is almost physically impossible that the surface water which supplies the superficial wells could be pure. In country districts this kind of water is occasionally very pure, owing to the absence of manure, or because the drainage is obtained from a thin and grass-covered soil. The deeper the well is the greater is the drainage area which supplies it. A shallow well often collects only the drainage of a few hundred square yards; in which case the liquid, not having been subjected to prolonged contact with the purifying clay, is almost certain to contain organic impurities in excess. When making a well these points should be fully considered. If the well be only six or eight feet deep, care must be taken not to have a cesspool or manure heap, on any part of the catchment area, for otherwise some oozings from the manure would be sure to get into the well. For this reason, if for no other, liquid manure tanks and cesspools should be so constructed as to prevent the leakage of their contents. They should also be placed as far as possible from the well—an observation which applies with equal force to sewers.

The quality of the water used in the majority of Irish small towns and villages is very bad. The "parish pump" collects not

only the rain water which falls on the catchment basin of the district, but also no inconsiderable amount of the liquid refuse thrown out from the houses and stables of the town. Many specimens of this kind of water have come under our own observation; and in several cases we were quite certain that they had been the means of spreading cholera when that disease was last epidemic in Ireland. Quite recently we examined for Dr. Tufnell the water supplied by one of the public pumps at Dalkey township, county of Dublin, and found it to contain the large amount of 70·26 grains of solid matter per gallon, of which no less than 22·06 grains consisted of "organic and volatile matters." These latter consisted chiefly of nitrates and nitrites, and therefore afforded the clearest evidence that the water had been largely contaminated with sewage impurities. Here we have the case of a well water obtained in a granite district, which in general yields exceedingly pure water, but which, owing to contamination from the refuse thrown out from human habitations, had lost all the characteristics of the water from primitive rock formations.

Mr. Bailey Denton, the eminent engineer, has suggested a method of supplying villages and small towns with good water and in sufficient quantities. He proposes to collect in reservoirs all the drainage from the subsoil when it has penetrated to the extent of about four feet. By this operation the land would be deprived of its excess of moisture, and reservoirs of very soft and wholesome water would be collected for the use of the villagers or townsfolk. In the case of a village containing 400 inhabitants, and requiring for each individual a daily supply of 10 gallons, a reservoir of $7\frac{1}{2}$ feet deep, and covering $\frac{4}{10}$ths of an acre, would be required. This pond would contain 720,000 gallons—a quantity which, making allowance for evaporation and waste in other ways, would afford 120 days' supply, without being replenished. The cost of a reservoir of this size is estimated by Mr. Denton at £415. Sanitary engineers and medical men have highly commended Mr. Denton's scheme. We fear, however, that in seasons of drought and heat the water of such shallow ponds as those proposed by Mr. Denton would become greatly deteriorated in quality. Perhaps it would be a better plan were a good watershed selected sufficient to supply the wants of a number of small towns and villages. The Corporation of Manchester, who have a magnificent water reservoir, supply villages twenty miles distant from the works at charges considerably under the cost of the water supplies provided by Mr. Denton's method.

The Commissioners of the smallest town or township, and, in the case of purely rural parishes, the Board of Guardians of the poor have the power of providing supplies of water for the inhabitants residing within their jurisdiction. The eleventh section of the Sanitary Act of 1866 confides this power to the above-named bodies, but it does not empower them to purchase land except by agreement, and in the manner prescribed by the Local Government Act of 1855. It is unfortunate that the latter Act does not extend to Ireland, for several of its provisions affect more or less the public health. The Corporation of Dublin are urging the Government to bring in a measure for the extension of the Act to Ireland; and we have no doubt but that the request will be complied with, and probably during the present session of the Legislature.

We have already stated that the water supplies of many of the larger cities are now derived from lakes and rivers. These are, in general, situated in thinly-peopled districts, where the waters are not liable to be tainted with sewage impurities. With few exceptions, the new water supplies contain very little solid matter in solution, whilst the water formerly in use was, as a general rule, *hard*—that is, it contained from ten to fifty grains of solid matters per gallon. When the bills for the better supplying of Manchester, Glasgow, Dublin, and other towns with water were before Parliament, the medical and scientific evidence educed was nearly altogether in favour of soft water as against hard water. It was to a great extent on the faith of this evidence that Parliament was induced to sanction the expenditure of several millions of pounds for the construction of water works. After all this it is rather startling to find a high sanitary authority deliberately stating that the use of soft water in a town greatly increases the mortality of its inhabitants! The authority to whom we refer is Dr. Letheby, medical officer of health for the city of London. In his report on the sanitary condition of London during the last year, he gives a table showing the average composition of the water supplied to the metropolis during the year, and from which we learn that the proportion of solid matter per gallon varied from 18·18 to 27·85 grains. The solid matter consisted chiefly of lime salts, which Dr. Letheby contends render the water wholesome. He says:—

"In this country it is remarkable that wherever soft water is supplied to the people the mortality is large, even when allowance is made for the birth-rate of the place. Glasgow, for example, as well as Preston, Dundee, Sheffield, Plymouth, Manchester, Bradford, &c., which are all

supplied with water of less than four degrees of hardness, have a mortality which ranges from 26 to 34 per 1,000, while at Birmingham, Bristol, Sunderland, Newcastle-on-Tyne, Wakefield, Dover, Norwich, Croydon, Worcester, Derby, and other places where the waters are hard, the mortality is considerably less—in fact, it may be said that in towns supplied with water of more than ten degrees, the average mortality is about 22 per 1,000; while in those supplied with softer water it is about 26 per 1,000."

Dr. Letheby states that the *savans* appointed by the authorities of Paris and Vienna to decide on the quality of the water to be supplied to those cities, reported in favour of water of moderate degrees of hardness. One consideration seems to have powerfully influenced the decision of the French *savans*—namely, the allegation that more conscripts are rejected in the soft water districts, on account of imperfect development and stunted growth, than in the hard. "And if," says Dr. Letheby,

"We regard the subject from another point of view, there is the striking fact that, as three-fourths of the earth's surface is covered with calcareous strata, we are compelled to drink calcareous water, as if it were intended for a wholesome purpose; and it may well be, as the late Professor Johnston observed, that 'the bright sparkling hard waters which gush out in frequent springs from our chalk and other limestone rocks are relished to drink, not merely because they are grateful to the eye, but because there is something exhilarating in the excess of carbonic acid they contain, and give off as they pass through the warm mouth and throat; and because the lime they hold in solution removes acid matters from the stomach, and thus acts as a grateful medicine to the system. To abandon the use of such a water, and to drink daily in its stead one entirely free from mineral matter, so far from improving the health, may injure it. In fact, the water of a country may determine the diet of its inhabitants. The soft water of the lakes of Scotland, for instance, may have had much to do with the use of brown meal; and but for the calcareous waters of Ireland the potato could not have become a national food. Looking, therefore, at the plain teachings of all this, and considering the excellent quality of the water supplied to this metropolis, it would be folly, in my opinion, to change it for a soft water."

The citizens of Dublin have recently expended nearly half a million sterling in supplying themselves with water of about two degrees of hardness instead of the water of from fifteen to forty-two degrees of hardness formerly in use in that city. If Dr. Letheby's view be correct, the inhabitants of Dublin may justly complain that, acting upon the advice of scientific men, they have been led

to worse than useless extravagance. To our mind, however, it seems inconceivable that the use of pure soft water could be the means of largely increasing the death-rate of a town. The ordinary food which we use contains abundance of mineral substances, with which the bony structures of the body are nourished. Even if they did not, the quantity of earthy matter taken into the body through the medium of water would be perfectly inadequate to subserve any useful purpose. Let us take the case of an average sample of the hard water supplied to London. It contains 16 grains of earthy salts per imperial gallon (70,000 grains weight, or 160 ounces). Few persons drink more than half a gallon of water per day, so that a person consuming that quantity of water would derive from it only eight grains of earthy salts. It must be borne in mind, too, that by heating water, the hardness of which is due to the presence of calcic carbonate (carbonate of lime), a large proportion of the earthy salt is rendered insoluble. Much of the lime contained in the hard water used for domestic purposes is deposited in the tea-kettles and other appliances of the household. The quantity of calcic carbonate in a man of 160 lbs. weight is about one pound, and an adult daily consumes in his food at least $\frac{1}{4}$ oz. of earthy salts. As the lime contained in the bones is associated nearly altogether with phosphoric acid and *not* with carbonic acid, it is evident that the chalk contained in hard waters cannot contribute, at least in an important manner, to the nutrition of the body.

The apparent relation between the death-rate and the nature of the water supplies in the towns mentioned by Dr. Letheby is certainly remarkable; but we should like to learn the death-rates in Glasgow, Manchester, and the other towns supplied with soft water when the water furnished to them was hard. Dr. Letheby includes Dublin amongst the towns which have a very high death-rate; and yet, until very recently, that city was supplied with water of about the degree of hardness which Dr. Letheby considers most desirable. So far as our observation extends, we are clearly of opinion that the substitution of the pure soft water of the Vartry for the hard water supplied by the canals, has produced an improvement in the public health.

We have received a letter upon this subject, from Dr. Gairdner, medical officer of health for the city of Glasgow, and author of an excellent work on air and water. We quote the following extract from Dr. Gairdner's letter:—

"The highlanders and islanders of Scotland—who despite many unfavourable circumstances—enjoy the credit of a low death-rate, almost all drink soft water; and I do not know a single fact showing that soft water, in the many instances in which it has now been brought into populous places, instead of hard, has in any instance led to an increased death-rate, or produced any appreciable bad effect where the other sanitary circumstances are good.

"In Glasgow the whole of the west-end well-to-do population drink Loch Katrine water, and do not suffer. The death-rate of the town is affected by quite other causes."

PROPAGATION OF CHOLERA AND TYPHOID FEVER.

At a meeting of the Association of Medical Officers of Health, London, held on the 19th February, 1870, Dr. George Buchanan read an elaborate paper on Pettenkofer's theory of the propagation of cholera. The paper is published *in extenso* in the *Medical Times and Gazette*, March 12th, 1870. In our Report on Public Health, published in the *Quarterly Journal of Medical Science*, November, 1869, we fully described Pettenkofer's newest views relative to the propagation of cholera. The correctness of these views is disputed by Dr. Buchanan, who, from his position of Government sanitary inspector, has had peculiar facilities for investigating the conditions under which cholera spread in England during the recent epidemic of that disease. Dr. Buchanan believes that cholera and enteric fever are communicated chiefly through the medium of polluted water. Pettenkofer states that when the soil water is lowered the cholera poison diffuses itself throughout the porous soil: but, according to Buchanan, when the soil water subsides, the superficial wells become more impure from effete animal matters; and the drinking water which they supply is the direct carrier of the disease. It is evident that when the soil water is nearly up to the surface of the ground the waste matters thrown out from dwellings are carried off horizontally by the drainage; whilst, when the soil water is low, the refuse sinks into the surface-wells, which thereby become polluted, and during epidemics of cholera, or endemics of enteric fever, retain the virus of those diseases. Dr. Buchanan quoted several facts to prove that typhoid fever is spread by means of polluted potable water; and he endeavours to prove that the subsidence of soil water does not affect the spread of the disease in localities supplied with water brought from pure and remote sources. We quite agree with Dr.

Buchanan in regarding impure drinkable water as a common vehicle in which the germs of several zymotic diseases are carried into the body. During the last epidemic cholera in Ireland we found that the pump water used in several localities where the disease was unusually severe, contained excessive amounts of animal impurities. This was particularly the case with Arklow and Mallow. In the latter town there were few cases of the disease except in one suburb, the well water of which we found loaded with organic matter, nitrates, nitrites, and ammonia. Where the disease did not prevail the water was remarkably pure. Dr. Buchanan's views are greatly strengthened by the facts set forth in Ballot's report on the spread of cholera in Holland; for that author has shown that in every town in the Netherlands, where rain water alone was drank, there were either no cases of cholera or a few isolated ones. On the other hand, where wells and canals furnished the water of towns or villages, the disease prevailed. Pettenkofer himself does not deny the fact that cholera and typhoid fever are occasionally contracted by drinking impure water. In November last we had the pleasure of conversing with Professor Pettenkofer upon this and other subjects, and he stated that he believed cholera was occasionally communicated through the agency of potable water, but that the general spread of the disease was not through that channel.

The most striking fact adduced by Dr. Buchanan against the most important part of Pettenkofer's theory is that, whilst main drainage works were carried on in seven large towns the mortality from enteric fever actually declined. These towns were supplied with water from a distance, and therefore the subsidence of the drainage only affected the wells, from which no important part of the water supplies of the town was derived.

"The following shows the mortality from enteric fever in each town before and during the years when the soil water was sinking, the figures representing in each case a death-rate per 10,000 of population yearly:

	Before.	During.
Bristol, - - -	10	8
Leicester, - - -	15	$10\frac{1}{5}$
Merthyr, - - -	$12\frac{2}{5}$	$8\frac{2}{5}$
Cardiff, - - -	$17\frac{1}{2}$	$4\frac{1}{2}$
Carlisle, - - -	10	$8\frac{3}{4}$
Macclesfield, - -	$14\frac{1}{4}$	14
Newport, - - -	13	$12\frac{1}{4}$

Of these towns the lowering of soil water was most considerable in the cases of Leicester and Cardiff. It may be well that I should guard against misapprehension by stating that, in the period after the execution of the works here considered, the reduction of mortality from enteric fever was, with scarcely an exception, greatly more conspicuous than in the time during which the works were being done.

"It appears, then, from the experience of these seven large towns, that when lowering of soil water is brought about, while drinking water is being supplied, not from the soil of the place, but from pure external sources, Pettenkofer's experiences, obtained from places which do derive their water supply from their own soil, fail of application. It is probable that if closer inquiry were made into the sequence of sanitary improvements, some or all of the towns which were first set aside might afford further evidence adverse to the general expression of facts given by Pettenkofer—at all events, none of these towns afford any colour to the supposition that sinking of soil water exercised appreciable evil effect against the good effects that were being gained in other ways.

"The three towns which I have left for further mention are Croydon and Worthing, where during the years of soil water reduction there was an increase of fever; and Stratford-on-Avon, where, with considerable reduction in the level of soil-water, great reduction of fever also took place, and this although the source of water supply remained what it had always been—the porous ground upon which the town stands. The case of Stratford is the only one which appears to me antagonistic to the whole of Pettenkofer's theory. Its fever death-rate was $12\frac{1}{3}$ yearly before drainage works were begun in 1854. The construction of the sewers reduced the level of the subsoil water, intercepting springs, and drying wells, many of which had to be deepened. Yet, while these works were going on, the annual fever death-rate was only $5\frac{3}{4}$. The other two instances, Croydon and Worthing, appear at first sight to bear out Pettenkofer's views in their entirety, for both of them, having pure water supplied to them from sources out of the reach of contamination, got their soil water much reduced during the time that certain drainage works were being made, and at the same time got a considerable increase of fever; in the case of Croydon from $16\frac{1}{2}$ to 19 per 10,000, and in the case of Worthing from $7\frac{1}{2}$ to $9\frac{1}{2}$. But in each of these places it is known that arrangements intended for the removal of excrement had for their result the free discharge of sewer gas into houses, and the evidence connecting the outbreaks of enteric fever with this defect is so conclusive as to obscure altogether any causative conditions relating to the soil.

"Admitting, then, Professor von Pettenkofer's thesis that subsidence of soil water is a condition favourable to the epidemic prevalence of

enteric fever, I think it has been shown that the qualification is wanted 'where in the town or place the supply of drinking water is derived from the soil upon which the town stands;' and then the conditions to which Pettenkofer calls attention will range themselves under the more general etiological head of circumstances assisting in defiling supplies of drinking water. I have not much doubt that what is true, in this regard, for enteric fever, is also, in its degree, true for cholera; but upon this point, remembering the varied character of the statements connecting cholera with soil, and feeling the need of wider and more exact experience, I cannot venture to speak with certainty."

Dr. Bryden, of the Bengal army, has recently published a voluminous report on the outbreak of cholera in Northern India in 1866-68. Some of the facts mentioned in this report are strongly in favour of the theory of the general propagation of cholera by means of impure water. Whilst the water-tanks are fouled by dipping clothes into them and bathing in them, and the supplies are drying up, the disease increases rapidly; but when the tanks are cleaned out and refilled with pure rain water the cholera vanishes, and does not reappear during the time that rain keeps the tanks full of water.

DUST AND DISEASE.

Professor Tyndall, in a lecture delivered in December last, has popularized a subject which previously was known only amongst scientific circles. The professor, in making some experiments with beams of light, found that the dust present in the air interfered with the results. This dust proved to be chiefly organic, and therefore combustible, and he destroyed it by passing the air through a platinum tube heated to redness. Professor Tyndall also noticed that the air passing into the mouth in the act of inspiration contained dust, whilst the air expired from the lungs was free from solid particles. As cotton wool absorbs and retains atmospheric dust when a current of air passes through it, Dr. Tyndall recommends persons who are obliged to be in contact with patients suffering from contagious diseases to wear respirators made of cotton wool. This advice is, of course, based on the germ theory of zymotic diseases; and it presumes that matters containing the seeds or virus of the disease are exhaled from the lungs and skin of the affected. Dr. Tyndall causes a ray of light to traverse the air. If it be visible to the ordinary observer it is because

germs of animals and plants (and perhaps of diseases), or other solid matters are present. Where the air is free from solid matters, there the sunbeam—or rather what is popularly termed the sunbeam—is not visible. In ventilating buildings by forcing a current of warm air into them, would it not be advisable to subject the air to a temperature sufficiently high to destroy any organic matter which it might happen to contain? In heating and ventilating such buildings as the Houses of Parliament, where expense is not considered, it might be found practicable to employ highly heated air, subsequently cooled to the proper temperature for breathing it.

Although the interesting lecture of Dr. Tyndall has directed general attention to the presence of organic matter in the air, the subject is by no means either new or but little studied. In one of our lectures on Public Health, delivered in May, 1868, in the Royal College of Surgeons, we exhibited the presence of organic matter in the air by passing the latter through a solution of permanganate of potash. In a few minutes the atmospheric organic matter decomposed the permanganate solution, the splendid red colour of which gradually faded into a yellowish hue. In a little work[a] of ours on Health, the following observations occur:—

"The organic matters in the atmosphere consist of a great variety of bodies. They comprise seeds, spores, pollen, minute vegetables, infusoria, insects and their remains, soot, fragments of linen, cotton, silk, and wool, hairs, particles of animal and vegetable substances. Minute traces of matters thrown off from animals exist in the air, and constitute the most dangerous part of the atmospheric organic matter. Pus has been detected in the air of hospitals, and epithelium from the skin may be found in the dust of most inhabited rooms. Chalvet collected the dust in a badly cleaned and ill-ventilated hospital, and found it to contain from 33 to 46 per cent. of organic matter.

"*Table showing amount of Organic Matter in Air.*

	Cubic Feet.
Pure air on high ground, 1 grain in from -	176,000 to 209,000
In a bed-room, - - - - -	„ 56,000
Inside a house, - - - - -	„ 16,000
In a closely packed railway carriage, - -	„ 8,000
Air of a cess pool, - - - - -	„ 62

[a] Lectures on the Preservation of Health (pages 47 and 48). London: Cassell, Petter, and Galpin. 1869.

"The experiments of Dr. Angus Smith clearly prove that the headache and other symptoms which we suffer from in ill-ventilated places are nearly altogether the result of the organic matter, and not of the carbonic acid, of the vitiated air. Hammond found that a mouse died in 45 minutes in air containing a large amount of organic matter, but no carbonic acid. Other experiments have afforded similar results."

At a meeting of the London Chemical Society, held on the 3rd February, Mr. Chapman read a "Note on the Organic Matter contained in the Air." The author found that the air of crowded rooms contains suspended nitrogenous organic matter as well as volatile organic bases. The first is removable by simple filtration through cotton wool; but the latter pass through filters, and when conducted through water can be detected therein. The air collected from the neighbourhood of a sewer contained decided quantities of these bases. Mr. Chapman suggests that it would be desirable to examine by this method the air of hospitals, fever wards, and similar places.

At a meeting of the Manchester Literary and Philosophical Society, held February 22, 1870, Dr. Arthur Ransome read a paper "On the Organic Matter of Human Breath in Health and Disease." The breath of eleven healthy and of seventeen persons affected by different diseases was examined. In both health and disease free ammonia was exhaled. Urea was sought for in the breath of three healthy persons and twelve diseased. It was found in two cases of kidney disease, in one of diphtheria, and but in the minutest quantity only in the case of a female suffering from catarrh.

A rough calculation was given of the total quantity of organic matter exhaled from the lungs in twenty-four hours. In the case of an adult it amounted to about forty-six grains. In kidney diseases a very large amount of organic matter was thrown off from the lungs. In a case of diphtheria confervoid filaments were observed; and in two other cases—one of measles, the other of hooping cough—numerous specimens of a small celled torula were detected.

The results of Dr. Ransome's experiments throw no light on the subject of the germ theory of disease. They show, however, the readiness with which the matters thrown out from the lungs support fermentation, and they afford a further illustration of the dangers arising from the ill-ventilation of hospitals. In concluding

this notice of the subject of Dust and Disease, we cannot but remark that Dr. Angus Smith of Manchester has done far more than any other chemist to elucidate the nature and to determine the amount of atmospheric organic matter in town and country, indoors and out of doors.

REGISTRATION OF DISEASE AND DEATH.

Last November a deputation, consisting of a conjoint committee of the Medical Society of London, the Association of the Medical Officers of Health, the St. Andrew's Medical Graduates, and the Poor Law Medical Officers Association, waited on Mr. Goschen, the president of the Poor-Law Board, to submit to him the basis of a scheme for the registration of disease. The plan suggested proposes that the usual weekly returns of illness made out by the ordinary poor-law medical officers, should be applied in such a way as would render them available for national, instead of purely local purposes. Each dispensary physician makes a weekly return to his board of guardians; a copy of this return might be forwarded to a central office—for example, to the office of the Registrar-General—where the facts contained in it, and other similar returns, would be classified, and published with reports upon them conveying valuable information to the local health authorities, and to the general public.

If a system of disease registration were initiated by the poor-law authorities, statistics of disease would very soon follow from hospitals, infirmaries, asylums, private dispensaries, medical relief associations, and from other sources: even private practitioners might ultimately be induced to co-operate in securing a really general national system of disease registration, which would be invaluable as a means of investigating the etiology of the zymotics, and perhaps of other maladies. The president of the poor-law board, Mr. Goschen, entertained very favourably, the proposal to establish a system of disease registration; and although it has not as yet been adopted, we have little doubt but that the practicability of the proposed plan will ere long be properly tested.

Mr. Watt, of Montreal, has in a recent pamphlet endeavoured to prove what he terms the fallacies of the Registrar-General's conclusions, with respect to the mortality tables of towns. He shows that the mere death-rate of a place is no criterion as to its salubrity, unless corrected by means of factors taken from the birth-rate. The immigration into, or the emigration out of, a town also affect

the conclusions deducible from the mortality tables. Dr. Letheby also attacks[a] the Registrar-General. He states—

"In the weekly and quarterly returns of the Registrar-General, the death-rates of certain British and continental towns and cities are so prominently stated, that the reader is apt to conclude they are the direct expressions of their sanitary condition. But nothing can be more fallacious; for the town with the lowest death-rate may be the most unhealthy, and so also conversely. To take, for example, the list of places referred to by the Registrar-General in his quarterly report, it would seem that Dublin with a death-rate of 22·72 per 1,000 of the population, must be considerably more healthy than Liverpool with a mortality of 27·29 per 1,000. In reality, however, it is the reverse, for if measured by the birth-rate as well as the death-rate, it will be found that Liverpool is in a better sanitary condition than Dublin. Taking, in fact, the average birth-rate (35·08 per 1,000), and the death-rate (22·58 per 1,000) of England as the standards for comparison, it will be seen that the actual death-rates of these places during the year which has just expired are very different from the calculated death-rates; and that little or no estimate can be made of the comparative salubrity of these places by mere reference to the observed death-rates.

Observed Annual Birth-rate and Death-rate per 1,000 *of the Population, and the calculated Death-rate.*

	Annual Rate per 1,000 Living			
	Births	Deaths	Calculated Death-rate	Difference of actual Deaths
Birmingham	35·65	20·35	22·94	— 2·59
Bristol	36·09	22·64	23·23	— 0·59
Leeds	40·86	27·26	26·30	+ 0·96
Sheffield	3·683	25·94	24·86	+ 1·08
Newcastle-on-Tyne	38·88	26·27	25·02	+ 1·25
London	35·48	24·13	22·83	+ 1·30
Bradford	37·94	26·03	24·44	+ 1·59
Salford	39·47	27·67	25·41	+ 2·26
Liverpool	36·93	29·07	23·77	+ 5·30
Manchester	35·91	29·15	23·11	+ 6·04
Edinburgh	37·64	30·63	24·23	+ 6·40
Berlin	41·78	33·61	26·89	+ 6·72
Dublin	25·93	23·81	16·69	+ 7·12
Glasgow	40·11	34·23	25·82	+ 8·41
England (average)	35·08	22·58	22·58	0·00
Austria ,,	40·45	30·85	26·63	+ 4·42
France ,,	26·68	23·27	17·17	+ 6·11

[a] Report on the Sanitary Condition of London for the year 1869. London: Lownds, Fenchurch-street. 1870.

"In the above list the towns have been placed in the order of their salubrity, as determined by a comparison of the observed and the calculated death-rates, beginning with the most healthy; and it will be remarked that the observed death-rate is always very different from the true death-rate—that Dublin, for example, which has an observed death-rate of only 23·81 per 1,000, is nevertheless an exceedingly unhealthy city—much more so than Leeds, with an observed death-rate of 27·26 per 1,000; for the latter, considering the number of children in the place, has a death-rate but slightly above the estimated death-rate (26·03), while the former with its small proportion of children, has a death-rate excessively above it (16·29)."

It is but fair to state, that in the full reports published by the Registrar-General, he points out that fallacious conclusions might be drawn from his statistics; and he explains the necessary corrections which must be made in comparing the mortality returns of towns, with those of the rural districts. In the weekly returns published in the newspapers, these corrections are not, however, given; and consequently the inhabitants of very unhealthy towns, the observed death-rate of which may happen to be low, conclude from the Registrar-General's returns, that their sanitary state is excellent.

With respect to the vital statistics collected in Ireland, it may truly be said that they are utterly valueless. A large sum of money is annually expended in collecting statistics of births and deaths; and we cannot but express our regret that so much of the public money is wasted in gathering and publishing statistics which serve only to mislead us. Last year the death-rate in Dublin amounted to 24 per 1,000 persons, living, whilst the births registered numbered only 26 per 1,000 persons. In some former years, the birth-rate was actually under the death-rate. Every physician in Dublin must know, that in it the births greatly exceed the deaths; and that the discrepancy between the actual birth-rate, and that recorded by the Registrar-General arises from the fact, that the lower classes do not, whenever it is possible, cause the births of their children to be registered. We believe that only a small percentage of the births of children born without their mothers having received medical assistance, is officially recorded in this city.

In the rural districts of Ireland, more especially in Connaught, registration of births and deaths is much neglected. A foreign student of vital statistics, reading the official statements of the

birth and death-rates in Ireland, would be astounded to learn from them, that the average duration of human life in Connaught, was eighty years!

The registration of births and deaths in Ireland ought to be either abolished or thoroughly reformed. The law making it compulsory to register those events should be rendered more stringent. No interment should be permitted unless the persons conducting the funeral produce to the keeper of the cemetery a medical certificate showing the cause of death. The police might be employed in ascertaining recent births and deaths, and in serving cautionary notices to have those events registered. Their services in this way might be annually rewarded; the bonus being proportionate to the number of cases registered in consequence of their information.

The fee received by the medical registrar for recording a death, is only one shilling—a remuneration absurdly small, and hardly adequate to pay for the mere scrivenery connected with the registration work. The sum paid for each registration should, at least, be doubled. It has frequently been suggested that it would be desirable to relieve the dispensary doctors from their functions as registrars of births and deaths. On this subject, we had recently occasion to make the following observations:—

"To me it appears evident that no satisfactory results will flow from any system of registration that is not exclusively managed by medical men. The duty of recording the births and deaths—I will say nothing of the marriages—should be entrusted to the poor-law medical officers; who, in return for their monopoly of this function, should be obliged to perform their work in a manner that would ensure its results being valuable to statistical and sanitary science. To any one who appreciates the advantages derivable from the study of accurate vital statistics, the peculiar fitness of physicians for the office of registrars of births and deaths is obvious. They would not commit the mistakes in the nomenclature of disease which the non-medical registrars sometimes make, thereby rendering their returns worse than useless. They alone would be able to indicate the causes of an unusual amount of mortality in their districts. As they constitute more than a third of the whole body of medical practitioners, and undoubtedly have charge of more than two-thirds of the sick, they have themselves the opportunity of ascertaining, personally, a large, if not the largest, proportion of the deaths occurring in their districts.

"I am quite satisfied that it is in the power of the dispensary physicians

who are also the registrars of deaths and births, to enlarge our knowledge of the etiology of disease—the first step towards its prevention or cure. We have a great deal to learn, not only relative to the ills which flesh is heir to, but to the flesh itself. Why is it that a disease extinguishes the spark of life in one man, deals gently with another, and passes by a third? I believe that the causes of death are seldom simple, and that in the majority of cases it is a bad constitution (*diathesis*), and not his malady, that kills the sick man. No doubt, it is useful to learn how many members of the community die annually from consumption; but the information would be far more valuable if it were known in how many cases the disease was the result of following an injurious trade, or of living upon a damp soil—whether it was produced from a direct cause, or was the result, mediate or immediate, of a constitutional tendency to phthisis. In short, the object for which vital statistics are collected would be more likely to be achieved if accurate information were obtained relative to the causes, proximate and remote, of deaths. Perhaps it would not be found impracticable to fill up some such form as that shown in the diagram.

"*Particulars to be stated in Form for Registration of Death.*

"Name—Age—Bachelor—Married or Widower—Birth-place—Diathesis—Occupation—Climatic and Telluric Conditions of Residence and Place of Business—Ages at, and Diseases of, which his relatives of the first degree had died—Disease or Diseases which directly or indirectly caused the death.

"It is, of course, not to be expected that so comprehensive a form as this is would, in the majority of cases, be filled up satisfactorily; but I am confident that the adoption of some such form of death registration would be the means of accumulating a mass of valuable medical statistics for the purposes of public hygiene."

The Lancet for January 1st, 1870, contains the following abstract of a paper by Pliny Earle Chase, read before the American Philsophical Society:—

"Mr. Chase shows that, notwithstanding the increased juvenile mortality, the Philadelphia life-tables indicate a possible life in Philadelphia of 114 years, a probable life of 33·44, and an expectation life of 35·09 years. He means by the term '*possible*' life, the limit sometimes obtained in a given locality; by the '*probable*' life, the age the probability of living beyond which is as great as that of dying before the age is attained; and by the '*expectation*' life he defines the average which will be attained by all who are born. In sixty-two years the average

mortality was 1 in 47·836, the coloured mortality in the same period being 1 in 27·763. The ratio of still births to total births was 4·3 per cent., and to total deaths 5·8 per cent. The ratio of living births to population was 2·8 per cent., and of deaths to births 74·5. The average natural increase was 3·3, and the increase by emigration 2·6 per cent. The main age at death was 23·57 years, and the main age of persons living was 24·29.

"But the most interesting facts in Mr. Chase's tables are those which show how the simple mode of life of a Quaker community compares with the life of a more active, or, rather, more luxurious people. He analyses the life-tables of the two communities of Philadelphia, dividing them into 'Friends' and 'Philadelphia,' and finds, as his results, that the Friends at the age of twelve years have a maximum vitality of 20·49 per cent. *over* their neighbours; that from twenty to sixty years of age they have a proportionate mortality of 23·37 *under* their neighbours; that their expectation of life is 24·62 per cent. higher, their probable life 43·78 per cent. more valuable, and their proportionate mortality at birth 44·70 lower than the mortality of their neighbours.

"The Quakers of Philadelphia approach thus towards the Jewish race, in respect of vitality, in which they are, probably, exceptional to all other Christian communities. The lesson brings us back in thought to those peoples of whom the student's classical and great master speaks: 'Plerumque tamen eam bonam contigisse ob bonos mores, *quos neque desidia, neque luxuria vitiarant.*'"

VACCINATION.

For several months past a single case of small-pox has not occurred in Ireland, and the loathsome disease may now be regarded as quite extinct in this country. One case did indeed occur in Dublin, but it was that of a sailor who came into port whilst suffering from the disease. The anti-vaccination agitation continues in England, but it is evidently on the decline.

The concluding paragraph of the annual report of the medical officers of the London Small-pox and Vaccination Hospital states that:—

"Considering, however, the prejudices which exist against vaccination, and the efforts which are now being made to foster them, it seems a duty incumbent on us, the medical officers of the small-pox hospital, placed as we are in *the* position the best adapted of any in this kingdom to test the prophylactic value of vaccination, to state emphatically that our confidence in vaccination is undiminished, and that each succeeding year supplies us with additional proof of its marvellous power in

preventing, modifying, and ameliorating small-pox, and this without any proof of those injurious effects, either proximate or remote, about which so much has recently been said."

ADULTERATION OF FOOD AMENDMENT ACT.

The Adulteration of Food Prevention Act, passed in 1860, has to a great extent proved a dead letter. The Act enables the corporations of boroughs, the commissioners of towns, and the magistrates assembled at quarter sessions, to appoint properly qualified persons to act as public food analysts. Up to the present, however, not more than half a dozen public analysts have been appointed in the United Kingdom. Even in towns where such officers exist, the general public do not, to any important extent, avail themselves of their services. The fees which public analysts are empowered to charge for the analysis of articles of food are 10s. as a *maximum*, and 2s. 6d. as a *minimum*. Of course it was presumed by the framers of the Act that public analysts would receive a fixed salary, which is actually the case in London, Dublin, and Birmingham. In Dublin the Corporation have fixed 2s. 6d. as the fee to be paid for the performance of an analysis of food or drink. About fifty analyses of food were made last year for the general public, but, with one or two exceptions, the persons for whom the examinations of adulterated food were made did not prosecute the vendors of the articles. The Lord Mayor's officers purchased a large number of samples of food, and the vendors of those found to be adulterated were prosecuted, and were fined.

Mr. Muntz has brought a bill into the House of Commons, for the purpose of amending the Food Adulteration Act of 1860, and for the extension of its provisions to drugs. It will, if adopted in its present form, be a stringent act. The fee for each analysis is fixed at a *maximum* sum of 2s. 6d. There are to be official collections of samples of food and drugs, and official prosecutions in the cases where those articles are found to be adulterated. The penalty for mixing, or ordering to be mixed, foreign substances with food or drugs, is to be a fine of £50; and on a second conviction of the same offender, he may be sent to prison for six months with hard labour.

The bill should render the appointment of public analysts compulsory on the part of local governing bodies, and it should not be indispensable that the officers must necessarily reside within the districts to which they are appointed. Let us take the case of the

County of Donegal, or that of the County of Roscommon, in neither of which there is a town containing 6,000 inhabitants : is it probable that a skilful analytical chemist, provided with a well-appointed laboratory, could be induced to establish himself in such a district, even though he were appointed county analyst at a salary of £50 or £100 a year ? If chemists residing in the larger towns were appointed public analysts for rural districts, samples of food and drugs could easily be sent to them for analysis by means of the " sample post " recently established by the postal authorities. Townships should have the power of appointing analysts. For example, the Town Commissioners of Kingstown, with its population of nearly 20,000, could not legally appoint a food analyst. This defect in the Act of 1860 ought to be remedied by the measure now before Parliament.

INFLUENCE OF STOVES UPON HEALTH.

The results of the experiments of General Morin, H. St. Claire Deville, Froost, Canet, and other chemists and physicians, appear to show very conclusively that iron stoves, by permitting the passage of the products of combustion through their walls, exercise an injurious influence upon the public health. The late Professor Graham proved that carbonic oxide gas was capable of passing through solid iron, provided the metal was heated to redness. As the front and sides of stoves are liable to become red hot, a large quantity of carbonic oxide generally escapes from the stove into the atmosphere of the apartment. The denser the fuel is, the greater the quantity of carbonic oxide formed during its combustion. Anthracite (in Ireland popularly termed Kilkenny coal), or flameless coal, and coke, produce more carbonic oxide than the bituminous, or flaming coal, and the latter develops more carbonic oxide than turf (peat) and wood. We may regard wood and peat as the fuels which, during their combustion, exercise the least injurious effect upon health. Dr. Derby, of Boston, Lecturer on Hygiene in Harvard University, United States, in a little work on " The Influence of Anthracite Fires upon Health," published late in 1868, states that the anthracite stoves used in America exercise a very prejudicial influence upon health. He recommends the following rules to be observed in burning hard fuel in cast-iron stoves:—

1st. To use fire-brick or soapstone (silicate of magnesia) in contact with the fire, and never to allow the fire-pot to get red hot.

2nd. To have perfect castings, and as few joints as possible; and these joints should be horizontal, and not vertical.

3rd. Never to cut off the supply of air to the fire, and never to check the free escape of the products of combustion to the chimney by dampers, or by cooling the fire by the admission of air between the furnace and the chimney.

4th. To so burn the coal that, under all possible circumstances, a pressure of air from without inwards may be exerted upon the fire-pot radiators and smoke-pipes.

In the number of *Les Mondes* for January 20th, 1870, Dr. Sacc asserts that cast-iron stoves, even if they are allowed to become red hot, are not injurious to health, provided that there is proper ventilation, and a good draught into the chimney—or, rather, into the pipe leading from the stove to the chimney. According to Sacc's experiments, cast-iron stoves are only injurious to health when rendered so by imperfect draught—a defect, however, which applies equally to all kinds of stoves and fireplaces.

INTENSITY OF LIGHT EVOLVED BY COAL GAS UNDER DIFFERENT CONDITIONS.

Mr. B. Silliman has published a very interesting paper on "The Relation between the Intensity of Light produced by the Combustion of Illuminating Gas, and the Volume of Gas Consumed," in *The American Journal of Science and Art* for January, 1870. Mr. Silliman shows, from actual experiment, that the same quantity of gas evolves, by its combustion in large burners, more light than when it is consumed in smaller ones. When a moderate and equally diffused light is required over a large space, it may be expedient to use a large number of small burners; but when a *maximum* intensity of light is desired, burners of ample flow are the best and most economical that can be employed. This subject is of interest in a hygienic point of view, for the less gas burned in our apartments the purer will be the atmosphere. It would be most desirable if the products of the combustion of the gas consumed in lighting theatres and other public places, were prevented from mixing with the air inspired by the audience. Gas might be burned in enclosed spaces, provided with glass sides to allow the light to pass through, and illuminate the apartment, and with a flue through which the products of combustion might be transmitted to the open air.

PART IV.

MEDICAL MISCELLANY.

Reports, Translations, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

Dr. Banks, President.

On the Employment of Blisters in the Bronchitis of Fever. By Henry Kennedy, A.B., M.B.; one of the Physicians of the Cork-street Hospital, Dublin.

That medical scepticism exists widely at the present day few will be disposed to deny. But all familiar with the history of medicine are aware that more or less of this has prevailed at all times: and, indeed, for myself, I have the strongest conviction that it will continue. There would appear to be some minds incapable of arriving at any definite conclusion—on such data, at least, as medicine affords, and can only afford. Still, as those minds constitute but a very small proportion of the profession, medicine, as a science, has received no injury from them; and so it continues to be a progressive, and, as I believe, an improving art. The work of the late Sir John Forbes was amongst the first of recent times to call in question the powers of our art over diseases. Before it, however, it will be recollected, that what I may call the blood-letting controversy had arisen—the most important question which has arisen in our own times. Since this, some of our most valued and long-tried drugs, such as calomel and tartar-emetic, have been denounced in no measured terms. The use of wine in disease has been ignored; and it is scarcely a year since it was gravely put forward in the leading

medical society of London that acute rheumatic fever was best treated by the administration of that potent agent, mint water. It will be recollected that I brought this question before the association at the time, and it had the effect of bringing out the almost unanimous opinion of the profession in Dublin that the alkaline treatment of the disease led, by much, to the best results. Within the last year, too, we have had the question raised in England, whether vaccination be a blessing or the contrary; and you are all aware what a controversy is going on at the present minute about it. Lastly, I hold in my hand a recent number of the periodical known as *The Practitioner*, which contains a paper by a French physician. The paper, too, has the strong sanction in its favour of the editor, and is headed, "On the Dangers of Blistering, especially in Pneumonia."

Now, in making these remarks I have had two objects in view—the first being to protest, in the strongest terms, against one and all of these medical scepticisms. I believe that medicines, like calomel, tartar-emetic, and alkalies, will continue to be used; that wine will not lose its place as a remedial agent; that vaccination will still be adopted; and that the theories advanced against blisters—and I must say they are all theories—or their dangers, as the Frenchman has it, will not prevent their being used as generally as they ever were. And this leads on, in the second place, to the more immediate object of this paper, which is, to make a few remarks on the value of blisters; though they have of late been so much decried.

The principle of counter-irritation, in its many forms, is, I need scarcely say, one of great extent and no little importance; so much so, indeed, that on the present occasion I must limit myself to a very small portion, if I may so speak, of the subject; and that is to the value of blisters, chiefly in one of the complications which arise so often in the progress of our fevers—I mean those of the chest.

In the treatment of fever it must be allowed that as yet we have little or no power over the disease *per se*. I am sanguine, however, the time will come when this will be accomplished; and that some antiseptic or alexipharmic, as it used to be called, will be discovered, which will prove to be antagonistic to the poison of the disease.[a] But it is far otherwise as regards the complications which arise, and which, in a very large number of instances, really place the life of the patient in danger, or lead directly to a fatal result. Over these, I presume, there are few who

[a] The medicine I at present use, and have the greatest confidence in, is barm. I am speaking here of spotted typhus. Under its use the spots very soon change from a dark to a red hue; and, what is of more consequence, the mortality has, I believe, been brought to the lowest on record. Having spoken on this particular subject on another occasion, I cannot allude to it further here.

will assert we have no control; even though this statement be in direct opposition to the often-expressed idea that in fever we can only support the strength of the patient, while the disease is passing through its stages, I find it hard to understand such a statement, or how, in making it, the complications of the disease have been so entirely overlooked. For myself, I must say that, as far as I have seen, cases of pure uncomplicated fever which have proved fatal have been very exceptional, whilst, on the other hand, complications arising in the course of the disease, and proving fatal, have been far in a way the general rule. Now, of those which occur the affections of the lungs are probably the most frequent, though, like other complications, very much more common at some periods than others; as, of course, every one familiar with fever is quite aware. And indeed these statements are what might be called truisms, so well are they known; but it was necessary for my purpose to state them here.

Of the affections of the lungs occurring in fever, bronchitis plays the most important part. It is by much the most frequent, for pneumonia is rare in comparison; and it is far the most serious, for it affects life much the most frequently; and, what is never to be overlooked, it is very difficult of cure. Comparing the bronchitis of fever with the pneumonia of fever, the latter is much the more amenable disease, and consequently the less serious. It is, however, far from my intention to say anything here of bronchitis as it arises in fever. It is enough to know that it occurs, and in both, or rather all the types of fever, for there are several, but more particularly in those known as typhus and typhoid; and though the bronchitis in these two assumes a somewhat different aspect, which ought to be recognized, I cannot stop to inquire into it here. When it arises, then how is it to be met? Besides other treatment—including chiefly stimulants, and, of course, wine—blisters, I firmly believe, will be found to be of the greatest value, and that life will often be saved by their proper use. Here I cannot but notice a practice which has of late years crept in, and superseded, in a degree, the use of blisters—I mean turpentine stupes. I do believe no more fatal mistake could be made in grave cases; for I hold that no stuping even approaches in its effects that of blisters, and that precious time will be lost by the former mode of procedure. I do not profess to enter here into the question how blisters act. I leave this to the editor of *The Practitioner*, who, not being able to advance a theory on the point satisfactory to himself, decides against their use. For myself, I prefer to be guided by experience, and the result is that I am diametrically opposed to the opinion of the gentleman of whom I speak. Were we to give up remedies because we cannot explain their mode of action, the treatment of disease would be turned into a farce.

But to render blisters effectual in the bronchitis of fever there are one or two points worthy of note. In the first place, they ought to be large. Obvious as this point is, I have reasons for knowing it is frequently overlooked. Parties forget the great extent of surface involved in most cases, and the blister, when ordered, is not at all commensurate with the extent of the disease; hence failure is too often the result, and even when relief does follow the small blister it does not conquer the disease. Size, then, I take to be a most important element in the use of blisters in those cases; and by size I mean here one that would cover the whole front or back of the chest. Of the two surfaces I believe the latter to be the best; but we must often have recourse to both to save our patient.

Probably the meeting will allow me to introduce here a few cases given in the briefest way, in which blisters used as I have advised were the chief means of saving life. Cases given as examples may, in this way, impress more forcibly the points I wish to convey.

Case I.—Crickkard, a girl aged twenty-two, admitted to hospital labouring under fever of a low type. In the course of three days she became spotted, or rather blotches of a very irregular shape appeared; some much larger than ordinary petecchiæ. In this state, and when now about ten days ill, bronchitis came on, and increased to the most serious degree, the distress being very great for more than a week; the pulse reached 140, and the hands and lips were livid; and, what showed more than anything else the danger, was the fact that the brain, which had been heavy and stupid, became clear and natural, and alive to the danger which existed. By powerful blistering this case came round; nor did she get more at any time than ten ounces of wine in the twenty-four hours.

Case II.—Brophy, a girl of twenty, sent in by Dr. Mollan; passed through a very severe attack of enteric fever, during which a good deal of bronchitis existed. After improving a little from this state she relapsed, and the bronchitis became the most serious possible. The pulse rose to 140; the lips got livid, and the distress for three nights was extreme. During this period she was powerfully blistered; and it was particularly noticed, that while the general treatment was the same, the relief obtained and expressed by the patient was most marked; and she finally, though after a very severe struggle, recovered.

Case III.—Martin, a boy of twelve years of age, admitted to hospital labouring under typhoid fever, well marked. His chest, too, was early engaged, his lips livid, and his distress from dyspnea very great. The pulse was very rapid, and there was constant raving. In this state his life was in the balance for many days. Finally, by relays, as I may call

them, of blisters, the chest symptoms gave way, and the patient recovered.

Case IV.—O'Neill, a girl of sixteen, passed through a severe attack of fever, with spots, during which her life was in the greatest danger from an attack of bronchitis. Her pulse became excessively rapid. Under powerful blistering this case also came round; and, I would again observe, without any change in the general treatment.

In this way a large number of cases might be given. But such is quite unnecessary; they would be similar to those described, and only agree in the point of all being most urgent from the lungs being engaged. It is particularly to be observed, however, that even when they proved fatal, as they often did, there was the clearest evidence that, in a degree, they had been of use; for it was quite obvious to myself, as shown by the lessened labours of the respiration, and the patients themselves often expressed relief, even though the case ended fatally. It is, of course, well known that our poor frequently will not come into hospital till the urgency of the symptoms forces them; and in no case is this more common than in the very complication of which I now speak. And, in fact, too many of them are admitted only to die. This leads on to a second point in connexion with my subject, which I must notice, which is the great value of blistering quickly. I am satisfied that this is an important principle to keep in mind, and that it may save life, where ordinary blistering, to even the same extent, will fail. It seems to give a kind of revulsion or shock to the system often beneficial; and I cannot help repeating, that the poor patients themselves have often at the time expressed relief from it. Hence in these cases I am in the habit of having the chest at once rubbed freely with the acetum cantharides, and this immediately followed by a blister. By this mode a discharge is rapidly induced, and it is the discharge which benefits the patient. I shall just give one case bearing on the point.

Case V.—Mangan, a man of fifty-five years of age, admitted February, 1863, in an advanced stage of typhus fever, being much spotted. He had all the appearance of a man about to die. He had constant raving, and was passing under him; but by far the most serious symptom about him was a very extensive bronchitis engaging the minute tubes in both lungs. In this state, besides stimulants, he was blistered in the way described; the entire chest being blistered in the course of twelve hours. I must say I was as much surprised as I was pleased to find this man a shade better the next day; and it is enough for my purpose to add that after a fearful struggle he came round.

There still remains one point to be noticed in connexon with my

subject. It is by no means uncommon to meet cases where, though life is not in immediate danger, still the patient does not recover. More or less fever remains, as well as bronchitis, and the pulse keeps much above the ordinary standard. In these cases marked benefit has arisen from converting a blistered surface into a kind of temporary issue. This has been done by the use of a very weak tartar-emetic ointment, which, however, requires caution. Twice ugly marks were left after the parts were healed. A safer plan is the use of one or, occasionally, two Albespiere's papers. It has been, in cases of enteric fever especially, that this mode has been found useful, where, as is well known, signs of hectic and even phthisis are very apt to arise. I cannot doubt I have seen life saved in this way; and were it necessary a large number of cases could be given bearing directly on this point. But these would exceed my present limits. It is right to add, that the blisters are only allowed to remain on four hours; that is, speaking generally of them; and when it has chanced that they have not risen well they have been re-applied over the same surface. It is also well to state, that though they have been used in very many cases of fever of the lowest type, as shown by the presence of a large crop of dark petecchiæ, no casualty worthy the name has occurred, nor have I ever seen the surface slough. In conclusion, then, I would just re-state the three principles, in the use of blisters, brought before you this evening, believing them to have a special practical bearing on the successful treatment of the bronchitis of fever:—

1. That the blisters ought to be much larger than those commonly used.

2. That in many instances rapid blistering is followed by the most decisive results.

3. That the conversion of a blistered surface into a temporary issue is often very beneficial.

P.S.—In *The Practitio er* for March, 1870, I observe the editor has again brought forward the subject of counter-irritation. The entire paper is occupied with the theory of the matter. But in the course of it he speaks of the danger to life, not only from blisters but even from turpentine stupes. There must surely be some error or exaggeration in such a statement.

Polyuria in some Forms of Chronic Renal Disease, with Remarks on the Physiology of the Kidney. By J. Magee Finny, M.B., Dub.; F.K. & Q.C.P.I., &c.; Demonstrator of Anatomy in the School of Physic, Ireland.

Having for some time past had my attention and study directed more particularly to the consideration of diseases of the kidneys, the symptom of polyuria in cases of chronic renal disease attracted my notice as

being, not only a very remarkable symptom, but one difficult of explanation. Many of these cases were proved, by *post mortem* examination, to be examples of the small contracted cirrhosed kidney, with its cortical portion so shrivelled and degenerated that the wonder was not so much that a large quantity of urine was passed per diem, but that the possessor of such altered and impaired organs could have survived so long.

For the explanation of this secretion of urine, either equal to, or in excess of, the normal average per diem in kidneys so diseased, I have referred to most of the books written on the subject within my reach, including the standard works of the modern times, but failed, in many instances, to obtain any information whatever, and in those in which this symptom was treated of, to derive much benefit, or to clear up the difficulty I experienced, a difficulty I feel confident has been shared in by many others. This brought me to examine for myself more closely the physiology, pathology, and histology of these important depurating organs as at present known, and the result of my study has been to assure me—and in this assurance I am not alone—that we require much correction and revision in our ideas on this subject, and that the theory so universally accepted and taught of the mode of secretion of the watery constituent of the urine needs to be, if not modified or entirely altered, carefully investigated and examined before being thoroughly accepted, since its tendency is, I believe, to retard us in the right comprehension of those pathological changes so often presented in chronic Bright's disease.

Since the general symptoms of the cirrhosed and the amyloid kidneys—the two forms in which polyuria is observable—are so well known and clearly defined, I shall not delay to dilate further upon them than to lay stress upon the almost invariable increase of urine which obtains at some period of the diseases alluded to. Neither shall I enlarge upon their morbid appearances, but call attention to the remarkable fact, that in both these kidneys the cortical portion is that which is chiefly and primarily diseased: in the one, due to the amyloid substance engaging at first the Malpighian capillaries (as definitely determined by Dr. Grainger Stewart[a]), and from that invading the vascular plexus and the tubules composing the cortex; in the other, due to atrophic degeneration, progressing in some cases to such an extent that the bases of the pyramids reach to the capsule; and so in each, though differently attained, the Malpighian bodies to a great extent—those very structures to whose agency we are taught to look for the elimination of the watery constituent of the urine—and the tubuli uriniferi in the ends of which these bodies lie, are rendered impervious and useless.

Polyuria as a symptom, and that a prominent one, of the contracted

[a] Bright's Disease, p. 67. 1868.

kidney, does not seem to have attracted the attention, nor received the consideration it deserves at the hands of the many writers on this subject. Indeed, in searching through the multitude of literature with which kidney disease is covered, I was much struck by the rarity that any notice whatever was taken of it as a symptom by no means easy to explain.

Dr. George Johnson, one of the few whose work alludes to it, suggests the probability, "that when the tubes continue to be nourished after the removal of their normal epithelium, this is associated with some modified secretion, or growth within them. I have sometimes thought," he writes,[a] "that the abundant secretion of urine of low density, which is so commonly associated with this form of renal disease, might be in part explained by supposing that some of the tubes when denuded in the manner described, having lost their normal epithelium, and with it the power of secreting the solids of the urine, may yet continue to pour out a watery secretion, which would dilute the urine and diminish the density. Another explanation of the abundant flow of watery urine is one which," he continues, "I have before suggested (when speaking of this symptom occurring towards the convalescence of acute nephritis after scarlet fever), viz., that the particles of epithelium which are thrown into the tubes stimulate the Malpighian corpuscles and excite them to pour out a current of liquid, the purpose being to wash away the debris and clear the tubes."[b]

The second whom I shall quote from, and whose opinion must carry considerable weight, is Dr. Roberts.[c] In his valuable and able work on urinary and renal diseases, alluding to this point, he writes—"It is a curious circumstance, that in several organic diseases of the kidney in which the renal substance is gradually destroyed (atrophic, Bright's disease, cystic degeneration, double hydronephrosis), the volume of the urine is sometimes increased though the solid matters are diminished. This appears to be an attempt on the part of nature of a compensating character, to maintain by *excessive transudation of water* the depurating functions of the kidneys under failing anatomical conditions."

The last I shall refer to is Dr. Grainger Stewart, who has so recently written on the subject. In answer to the inquiry, "How does it happen that while much of the kidney is atrophied, a natural or even excessive amount of fluid transudes?" he suggests,[d] "It may, perhaps, be explained by the fact that increased blood pressure is exerted on the Malpighian bodies which are still in action; but such an explanation is unsatisfactory, inasmuch as though it might account for the urine being not diminished, it cannot account for the actual increase. There may

[a] Diseases of the Kidneys. 1852, p. 214.
[b] *Ibid.*, p. 114.
[c] Urinary and Renal Diseases. William Roberts, M.D. 1865, p. 21.
[d] *Op. cit.*, p. 127.

be some change," he continues, "in the capillary walls whereby excessive transudation may occur; but such a change has not yet been discovered. It is not unlikely that in many cases the amount of urine is regulated by the amount of fluid consumed, but this cannot be accepted as the sole explanation, seeing that in one case experiments were carefully conducted from which it appeared that diminution of the supply of fluid ingested did not correspondingly influence the secretion of the urine. Perhaps," he goes on to say, "the true cause may one day be found in some abnormal condition of the blood, whereby its watery parts more readily permeate the capillary membrane." Again, in his remarks on the albuminoid kidney, Dr. Stewart takes the same view as to the cause of the polyuria. "This symptom," he writes, "appears referable to the degenerated state of the vessels. It may be that the degeneration of the muscular fibres of the small arteries leads to paralysis and dilatation of these vessels; but certainly we are entitled to assume, that the degenerated walls, although thickened, permit undue transudation of their contents."[a]

In seeking a solution of this question of polyuria, I conceive we are looking in the wrong direction for the light when the Malpighian bodies are viewed as the sole, or even as the chief, factors in the secretion of the watery constituent of the urine, although in making this statement I know I am at issue with the most distinguished and most talented authorities on the subject. There seems to me to be no occasion for accepting, as explanatory of the phenomenon, the stimulating action of dead epithelium on the Malpighian bodies, or the altered functions of living epithelium (as Johnson supposed); nor the theory of the compensatory secretion of water for solids (according to Roberts); nor, lastly, the exalted action of the Malpighian bodies and increased permeability of their walls, when the vessels forming these bodies are thickened to two or three times their normal size, or degenerated into an amyloid substance, which, I think, we have no reason to assume with Dr. Stewart is more permeable than the tissue composing the wall of healthy blood-vessels. A simpler explanation must, I feel confident, exist, and warranted by the advanced teachings of anatomy and physiology, such an one I would venture to suggest; while I am sure that, not one, but several causes combined, are at work in the production of this symptom of chronic renal disease. I believe the difficulty (and that there is a difficulty in accounting for this diuresis the explanations detailed above, by their variety and, as I read them, their want of sound worth plainly testify) is due in a great measure to our looking upon a small part of the kidney as though it were the whole kidney, and by our attributing to this part offices and functions shared in common by the whole.

[a] *Ibid.*, p. 90.

There can be no doubt that the obstacles in the way of the investigation of renal disease are many and great, both for experimental and microscopical study, and ought to be borne in mind in observing the inaptitude, and I may say the sluggishness, with which any new point is examined into by most men; and this I say, without, in the smallest degree, advocating that questionable thirst for novelty and claptrap so prevalent in the literature of the present day.

Moreover, as Dr. Roberts justly remarks,[a] the diversity of opinion and discrepancies between the descriptions of the various authorities of the morbid anatomy and the structural changes produced in the kidney in Bright's disease can only arise from, on the one hand, the uncertain state our knowledge is in with respect to the normal anatomy and physiology of the organ; and on the other, to attributing to diseased action (as Dickenson[b] lays stress upon) minute changes in the tube contents and in the epithelial cells themselves, which may all be found as occasional phenomena in kidneys in every other respect both functionally and anatomically healthy.

The researches within the last few years of Henle, Ludwig, Virchow, Schweigger-Seidel, and other German inquirers, and still more recently, of Beale, Dickenson, and Southey, make it abundantly evident that the description of the anatomy, as given by Mr. Bowman, and copied into every text book since then, must not be accepted entirely without modification or addition, and show that the course of the uriniferous tubules or of the blood-vessels is very far from being so simple as has usually been supposed.

In order, therefore, to place us in a right position to understand the physiology of the kidney with special regard to the water secretion, and, therefore, to read aright the changes produced in this organ by the diseases under consideration, I shall briefly refer to those discoveries in the tubular and vascular arrangements which may be considered as now generally recognized facts, and which bear directly upon our subject.

And here I would gratefully acknowledge the assistance a paper by Dr. R. Southey, in "St. Bartholomew's Reports," has given me, which I recently came upon, some time after I commenced this communication.[c]

The tubuli uriniferi, which by Henle, in 1862, were considered to consist of two distinct sets—some *open*, ending in a papilla; others *closed*, including the Malpighian bodies, but having no communication with the rest—have been proved, and, I may say, now positively demonstrated by Ludwig, Beale, and others, to be one and the same tube, only under different conditions and in different situations, confirming so far

[a] *Op. cit.*, p. 297. Note.

[b] Path. and Treat. of Albuminuria, 1868, p. 2.

[c] Minute Structure of the Human Kidney. St. Barthol. Reports, Vol. i., 1865, and Vol. ii., 1866.

the views of Bowman. Following the course of the uriniferous tube, beginning in the Malpighian body, we find that it immediately falls into a knot of windings and convolutions; emerging from these, it bends downward towards the pyramids, and following a straight course descends into one of these to a variable depth; it then turns and reascends to the cortical part; here, after a second and similar series of convolutions, it finally bends down again straight into the pyramidal portion, when it unites with other straight canals; and at last, greatly enlarged, it reaches the open mouth of a papilla.

The vascular supply to the kidney, in its component parts, is still more important clearly to understand, though not less complex and difficult. The vasa recta are those vessels about which the greatest interest lies, and with which I shall confine myself. The opinion of Bowman, that they arose from the efferent vessels alone has not been confirmed altogether by the subsequent investigators. For while some vasa recta, no doubt, and that in no very small quantities, come from the efferent vessels, there seems to be a very general conviction that, on the other hand, the arteriolæ rectæ spring direct from the renal artery without any intervention of the glomerulus in the Malpighian body. Virchow[a] originally made this discovery of a distinctly different origin for the vasa recta, and of a free anastamosis between the two systems at the base of the pyramidal portion. Dr. Beale confirmed Virchow's statement in 1856, and in his most comprehensive work, republished last year,[b] he expresses himself convinced of the correctness of this conclusion, after having fully re-examined the subject. Similar views are maintained by Ludwig, Hyrtl, Arnold, Dickenson, and Southey. The latter considers that in a healthy kidney the vasa recta spring mainly from the efferent vessels (agreeing thus with Bowman's and Berres' views), and also, in a smaller number, from direct capillarization; but that in a diseased state, more particularly when the vessels forming the Malpighian bodies are obstructed, and to this particularly I call attention, the vasa recta, which spring from the renal arteries, become very large and distinct. For example: in the amyloid kidney (from the examination of which Virchow has made his observations, and drawn conclusions somewhat, perhaps, too dogmatic), and in a similar specimen in Dr. Southey's possession, where a tubercular growth obstructs the vas ascendens to the lobule, the vasa recta are brought to view distinct and enlarged. That a similar change takes place in the cirrhosed kidney when the adventitious tissue compresses the Malpighian bodies is most probable, although I regret my inability to speak positively on the subject, notwithstanding having made several microscopic examinations with reference to this point. From these facts, then, it may be legitimate to assume that the medullary portion of the kidney, consisting as it does

[a] Archiv. f. Path. Anat. and Phys., Vol. xii., p. 310.
[b] Kidney Dis. Urin. Deposits. 3rd Edition, 1870.

of tubuli uriniferi, both looped and straight, will receive through the vasa recta a considerably greater supply of blood in all instances when the cortical intertubular region is impaired and the Malpighian capillaries obliterated, than in a state of health, or in those morbid states where the disease is principally confined to the epithelial lining of the tubes, as, for example, in acute or chronic desquamative nephritis.

There is, I suppose, no question on the mind of any as to the convoluted tubes being the chief *locus* of the secretions of the mass of the solid constituents and pigment of the urine, although Ludwig, Kölliker, and more recently Dr. Isaacs, of New York,[a] have claimed for the Malpighian bodies the office of the secretion of urea. There is not, however, much thought usually bestowed upon the medullary portion; and yet, when we bear in mind the close and intimate relation of the blood-vessels to the tubules in the pyramids, many of these having direct origin in the renal artery, the tubules themselves taking the peculiar course described above, and remember that the epithelium of the looped tubes is similar to that of the convoluted, the question is forced upon us, What is the function of this extensive tube territory; what part does it play in the secretion of the urine? These tubes must be something more than mere sewers or pipes laid to carry off the solids washed down, as some suppose, by the water charged with oxygen transuding from the Malpighian bodies—must have some higher function than calyces, infundibula, or ureters. I claim not for this portion of the kidney *as* high or *as* important offices as the cortex, nor fix upon it any peculiar property unshared by the rest, but I do claim for it higher qualities than ordinarily accorded it, and consider it a most important part of the organ by which compensatory work may be performed, either temporarily or permanently, according as the cortex be temporarily or permanently disabled, and upon the healthy condition of which, rather than of the cortex, the life of the individual seems more intimately to depend.

I believe the secretion of the urine, more particularly its fluid portion, takes place as much in the medullary region as the cortical, and is in reality the product of the active agency of the epithelial cells lining the whole uriniferous tubules.

The different character of the epithelium in the convoluted and straight tubes may be urged as an objection. But I do not consider it a great difficulty; for, though the epithelium lining the tubes opening on the papilla and those in the cortex are different, yet all through the pyramids the epithelium presents more of a transitional character, and is decidedly of a spheroidal type, verging to, we may say, and yet distinct from, the squamous character which it becomes near the papilla.

Further, I would say that although we constantly look to the spheroidal

[a] Trans. of New York Academy, Vol. i.

epithelium as the active agents of secretion, we have no right altogether to deny similar properties, though limited, perhaps, to epithelium somewhat less glandular, for, as Bowman observes, it is extremely hard to draw the line of demarcation between secreting epithelium, and that merely protective.

The theory of the kidney being but a filter of either solids or fluids, I, for my part, repudiate, notwithstanding that such views are to be found in some standard works on physiology and on diseases of the kidney. On the contrary, I state it as my belief that the water is as much a secretion as the solids, and for its secretion we must look to the epithelial investment of the tubes.

Against this view it may be urged, that the water of the urine is eliminated in a manner analogous to the filtration or transudation of water through the skin. Although, as Professor Huxley[a] puts it, perspiration would still continue to take place, to some extent, even in the absence of sweat-glands, as a process similar to what occurs in the lungs, the comparison will not hold good, I believe, when applied to the kidneys, since in them we lack the differing temperature and the differing specific gravity of the fluid within the capillary vessels, and of the surrounding medium whose hygrometric conditions are hourly undergoing variations, which obtain in the case of the skin.

Further, I would say that *osmosis* ought not to be applied to the elimination of the water at the Malpighian bodies, for that process, as I understand it, requires two fluids miscible, but of different specific gravity, to be separated by a thin septum of animal membrane gas or fluid, through which they pass until the specific gravity is equalized at either side. Now, in the kidney, although I believe osmosis may, and most probably does, take place from the vessels, forming the plexus around the tubes of the cones, into the tubules, at the Malpighian bodies at least, since the necessary conditions are absent, an exosmosis from the blood can hardly take place.

Again, if in the kidney, at the Malpighian bodies, the water of the urine be merely a filtration from the blood, as is universally taught, it is the only instance in the body where such takes place. I know of no other example where, under high pressure from behind, the liquor sanguinis can be drained of its water without some of the plasma passing along with it. Should such a condition of arterial pressure or venous obstruction be morbidly or accidentally set up in any part of the body, even in parts where the blood-vessels are few and large, we know albumen is certain to be found in the effused serum.

Let us, however, for a moment suppose such to be the normal course of things, that 50 oz. of water are naturally drained off per diem from

[a] Elementary Lesson in Physiology. Huxley, p. 129.

the blood, and are we not compelled to ask, what is it that gives this membrane, the capillary wall in the Malpighian body, so thin and delicate, the power of retaining some matters of the plasma and allowing only water to permeate its walls. But supposing, again, such power is really given the capillary membrane—although totally inexplicable to me—while the tissues of the kidney are normal and unaltered by disease, how is it possible that water can still permeate their walls in even a greater quantity than before, when these vessels are obliterated and destroyed, or the tubes into which they are supposed to pour their fluids are impermeable. Are not these great difficulties, and does not the filtration theory entirely fail to meet them?

If we turn our consideration now, for a few moments, to the lessons derivable from analogy, we will find, I believe, additional reasons for rejecting the theory that to the Malpighian bodies belong the special function of separating the watery constituent of the urine, inasmuch as we shall see that in the kidney, at our very hands, so to speak, lie the very same structure capable of secreting water which exist in other glands, and which all bear the type of unity so visibly stamped upon them by the hand of the Great Creator.

The amount of healthy urine secreted in twenty-four hours, averaging 40–50 oz., is not of such magnitude as to raise, in my mind, any difficulty in referring its source to the epithelium lining the tubules convoluted, looped, and straight. Dr. G. Johnson, however, considers the volume of water entering into the urine as something very difficult to account for, and, in fact, as something very peculiar to these organs. He says,[a] "The secretion of the kidneys differs from that of all other glands in the abundance of water it contains; and," he goes on to say in explanation, "that this water is eliminated by the Malpighian bodies, the singular position and all the details of the structure of these bodies, render in the highest degree credible."

Now, on referring to *Carpenter's Physiology* (6th Edit.), I find the amount of fluid secreted by the liver was estimated by Nasse and Platner to be, for a man of eleven stone weight ℥40 f., or $2\frac{1}{2}$ lbs.; the same amount is also given by Dr. Flint,[b] while Bidder and Schmidt give a still higher estimate, 56 oz. or $3\frac{1}{2}$ lbs. Again, saliva, which varies under the varying wants of the body and the supply of fluid ingested, may be set down at 2 to 3 pints in 24 hours.

Again, with regard to the cutaneous secretion and transpiration, the amount of fluid, according to Funke, over the whole body, fluctuating from 2 oz. to 29 oz. per hour, is on an average equal to $2\frac{3}{4}$ lbs. in the 24 hours. The last other secretion I would refer to is that of the stomach, which is really astonishing, amounting in a healthy man to 10 or 20

[a] *Op. cit.*, p. 59.

[b] Phys. of Man. Dr. Austin Flint, jun., 1870.

pints in 24 hours, while under favourable circumstances its maximum in one hour may not be less than 6 or 8 pints.[a]

These examples cited, which although not all glands of like functions or excrementitious glands, nevertheless present, with the exception of the Malpighian bodies, similar anatomical arrangements as the kidney, are fully sufficient to show that the amount of normal urine is not anything very peculiar; nor need it be considered a startling presumption to refer its secretion to the epithelium of the tubes, especially when we bear in mind the size and complexity of the kidneys, whose tubular surface has been estimated by Valentin at *six times* the whole amount of cutaneous secreting tissue, and reflect that the absolute quantity of blood traversing the healthy kidneys amounts, according to Brown-Sequard, to 2,000 lbs. per diem, or according to Professor Haughton, 566 pints.[b]

What is the arrangement of these glandular organs with regard to their vascular supply and epithelial lining? In the stomach, the salivary and the sweat-glands is not the essential parts exactly as in the kidney, namely, a tube lined by epithelium, glandular, columnar, and squamous, and having outside a network of capillaries in close and intimate relation. In none of the secretions alluded to is there any difficulty in accounting for the water which enters so largely, as we have seen, into them. We refer it, and rightly so, I believe, to nothing else than the vital electivity of the epithelium influenced by the amount of blood supply under nervous control. As, then, in the kidneys we find, I assert, the same structures in these glands, there seems no need to refer the elimination of the water to anything else than to them; nor does it necessarily follow, that because the Malpighian bodies present the peculiar features they do as to relation and structure, they are directly connected with this secretion; for if, with Prout, we accept the assimilation of water during the processes of assimilation, like the processes of assimilation themselves, to be a *vital* act,[c] I see nothing inconsistent with common sense and physiological data to ascribe the elimination of the water to a similar vital act on the part of the epithelium of the kidney.

That the Malpighian bodies have some peculiar functions which are of vital importance to the integrity of the kidney as a whole there can be doubt, but what there exact office may be it is not easy to pronounce dogmatically; but that it is not that of a water-filter seems still further confirmed by the very fact of their being found in considerable numbers in the urinary apparatus of birds and reptiles, into whose urine water, as a constituent, hardly enters at all. Kölliker, and more recently Dr. Isaacs, of New York, have maintained that urea is secreted by these bodies, but

[a] Diseases of the Stomach, Dr. Brinton, 1859, p. 32.

[b] Haughton on the Phenomena of Diabetes Mellitus, Dub. Q. Journal, Vol. xxxi., p. 377, 1861.

[c] Prout on Stomach and Urinary Diseases, 1840.

I see no reasonable grounds for supporting such a theory. More recently still, Dr. R. Southey has propounded a theory of their being *blood regulators*, and this theory I am most inclined to adopt, partly for want of a better, and partly because, on thinking it over, it strikes me as one having much of probability to recommend it. As his views may not be familiar to all, I shall quote his own words.[a] "All the blood," writes Dr. Southey, "passing through the kidneys makes its *premier début* on those little stages, but explains nothing of the *rôle* played out by them. No; there is something much more important effected at these blood-gates, which no engineer would have overlooked. Our explanations hitherto have been too depreciatory of Nature's handiwork. Through blindness we have ignored, perhaps, the most beautiful example of constructive design in the Creator's mind, which is presented in man his masterwork. Here is the sluicegate which commands supply; the balance wheel which regulates speed; the governor which prevents undue pressure; an escapement adapted to work for scores of years and not be worn out, or thrown out of gear."

In accordance with this theory the Malpighian bodies act as a sort of breakwater to the velocity of the blood, which otherwise passing with unchecked force into the capillaries surrounding the tubules of the cortex would most certainly rupture their thin walls, and hemorrhage into the substance of the kidney and the tubuli uriniferi, would be a phenomenon of constant occurrence.

There are, however, as we have already laid stress upon, other vessels which naturally act as safety valves to the Malpighian bodies, should the permeability of these bodies be temporarily obstructed, but which become enlarged, and to a certain degree constitute the chief channel by which the circulation of the kidney is maintained when these Malpighian bodies are rendered useless, and the vascular plexus beyond them impermeable by disease, namely, the vasa recta. Through these vessels, ramifying as they do among the tubules composing the cones, the blood, unchecked in its velocity on account of the impermeability of the cortex, will flow in a very full stream indeed, and, accordingly, it is most probable that so long as the obstruction to the cortical circulation lasts, and so long as the epithelium lining the tubes of the cones remains healthy, a rapid and large secretion of watery urine will take place in the region.

The loss of a regulating power, due most probably to a dilatation of the blood-vessels, produced (as Parkes remarks[b]) by paralysis of the constrictor nerves, is we know, a pregnant cause of diuresis.

In the state of great fear, in hysteria and other nervous affections, polyuria is well known to occur. Bernard's experiments of division of the sympathetic nerve in the neck, which was followed both by a diabetes

[a] *Op. cit.*, Vol i., p. 185.

[b] Parkes on Urine, 1860, p. 364.

insipidus and a great flow of saliva from the glands on the side operated on,[a] seem to place beyond doubt that the dilated state of the blood-vessels is followed by a watery secretion. Now, I believe that the same permanently dilated state of the vessels of the cones may be, and is mechanically produced when the Malpighian bodies are obliterated; and, accordingly, on the supposition, which I believe is correct, that naturally the tubes of the cones are actively concerned in the secretion of the urine, a rapid and large secretion of water will take place in this medullary region when the cortical portion is diseased or obliterated.

Having thus, to some extent, shown the views of the physiology of the kidney, which, I believe, may fairly be deduced from the more accurate acquaintance with the anatomical relations of the tubules and their vascular supply, and having endeavoured to show that Bowman's views cannot be accepted as to the secretion of the water of the urine, let us see how far the symptom under consideration can be reasonably explained; how the 2, 3, 4, or 5 pints of water can be secreted by organs so imperfect as the small contracted and the amyloid kidney; how it is that under these altered conditions the flow of water exceeds that of health in some stage of the disease.

To the due performance of the functions of any secreting or excreting gland three conditions, at least, must exist, exclusive altogether of nerve power, not that it is not a most powerful agent, but because in the case of the kidney our knowledge of the nervous influence is so slight, that we should have nothing better to rest upon but hypothetical theories. 1st. the blood, from which the fluids and solids of the secretion are to be derived, must be healthy both in quality and quantity; 2nd, it must be brought into close connexion with the elective epithelium of the tubes; 3rd, the epithelium must be itself in a normal state. The latter condition fails in acute tubular nephritis, as well as in the fatty degeneration of the organ; in the one, due to the turgid, too rapidly developed, epithelial cells, and fibrinous exudation into the tubules; in the other, to the epithelium rendered equally useless through fatty degeneration. It is thus easy to understand that in these two diseases the flow of urine should be less than normal, and such, by clinical experience, we learn to be the case.

In cirrhosis of the kidney, on the other hand, whatever epithelium does exist is normal; but the blood brought to the epithelium is considerably altered, and much deteriorated.

The following remarks upon this point have been chiefly compiled from Dr. Christison's statements, quoted by Dr. Johnson,[b] and from Dr. Owen Rees:[c]—

I. The serum is more abundant in proportion to the clot; the buffy

[a] *Ibid*, p. 100. [b] *Op. cit.*, p. 189. [c] Dis. of the Kidney, p. 40.

coat less frequently appears, and when it does, the clot is remarkably small and contracted; the fibrine is usually natural in its proportions, and only occasionally abundant when some local inflammation is set up.

II. Towards the period of the disease, when atrophy and shrinking of the gland substance is manifest, the serum is low in density and albumen is decidedly deficient. This deficiency of albumen in the blood is sometimes so marked, that the serum, when examined, is occasionally found so low in specific gravity as 1018, or even 1015, the standard of health being about 1029–1031. Very important changes are by this loss of albumen produced in the physical characters of the blood, the liquor sanguinis becoming watery, and leading to several secondary evils.

III. The hematosin gradually diminishes as the disease advances, and as length is reduced so much as to form less than one-third of the healthy average, so that, as Dr. Christison justly remarks, there is no chronic disease which so closely approaches hemorrhage in its power of diminishing the red particles of the blood.

Blood, then, of this poor watery character, is that fluid which usually in cirrhosis of the kidneys, circulates through these organs. Brought with greater force than normal to any remaining tubules in the cortical portion owing to the destruction, and of so many Malpighian bodies, brought with even still greater force, and in fuller current to the tubes of the medullary portion, a rapid secretion, we believe, of urine ensues; poor indeed in solids, and of low specific gravity, but in a quantity greater than normal, while its albuminous character may, no doubt, be attributed to some of the blood plasma having transuded the walls of some capillary vessels, whether at a Malpighian body or elsewhere, under the undue blood pressure.

Two other conditions are very often, also, at work to produce a still greater blood pressure on the vessels of the kidney, namely, the hypertrophied condition of the heart, and the dry and unperspiring state of the skin.

Strong action of the heart without evidence of valvular disease, is one of the commonest features of advanced renal disease, and was well known to Dr. Bright.[a] Whether, as that great physician puts the inquiry, the cause of the hypertrophy and strong action, be the altered quality of the blood irritating the left ventricle directly to increased action; or whether it be consequent upon the minute and capillary circulation, so as to render greater action necessary to force the blood through the distant sub-divisions of the arterial tree, as Johnson believes is the cause, I shall not delay to inquire, nor pretend to decide. The fact suffices, and, as Dr. Parkes remarks,[b] on the warrant of the observations of Ludwig,

[a] Guy's Hospital Reports, Vol. i.
[b] *Op. cit.* p. 99.

Goll, Dornbluth, and Heynsius, that other things being equal a strongly acting heart will furnish urine more abundant in water, and richer in solids, than a feeble heart, I can conceive, that although but a small portion of the kidneys is left, and that portion chiefly the medullary, yet, most probably, this strong action of the heart will tend, in addition to other forces, to the production of polyuria in cirrhosis.

In the amyloid kidney, polyuria is more easily accounted for, for besides the compensatory work, which we believe (for reasons stated above) the medullary portion of the kidney takes on it, it is extremely probable that through the Malpighian or other capillaries, a transudation of albuminous fluid takes place similar to the profuse watery diarrhea we occasionally observe in the more advanced stages of the disease.

On a Case of Malignant Disease of the Thorax and Stomach. By Thomas Wrigley Grimshaw, M.D. (Dub.); Fellow of the King and Queen's College of Physicians; Physician to, and Lecturer on Materia Medica in, Dr. Steevens' Hospital; Physician to Cork-street Fever Hospital.

The chief interest of the following case arises from the difficulties presented in determining the true nature of the affection under which the patient laboured during life.

The patient was a well-known medical practitioner of the southern suburbs of this city, aged fifty-one years. His previous medical history presented nothing particular, excepting an attack of fever during the great famine fever. He occasionally suffered from severe bilious attacks, and latterly from bronchitis. About two years ago he received a slight knock on the shin; this was followed by sloughing, and left an unhealthy ulcer, which for a long time remained unhealed. For this affection he was treated by the late Mr. Maurice Collis. The indolence and almost malignancy of this sore pointed to an impaired constitution. I may say his health decidedly failed from this time; he began to suffer from a constant cough, aggravated by attacks of bronchitis, and considerable irritability of the stomach. He had a great objection to have his chest examined, and indeed it was not examined until the disease was far advanced. By the advice of his professional friends he tried quiet and change of air—went to the County Wicklow, North Wales, and several other localities. These changes produced but temporary relief, and towards the end of last summer, after returning from a visit to North Wales, he became decidedly worse, got a severe attack of bronchitis, accompanied by great irritability of stomach. Up to this time (the end of August, 1869) he had been treating himself, sometimes taking the suggestions of his professional acquaintances. At this date he sent for

an eminent physician in this city, who pronounced his case to be probably tubercle of the left lung, softened at the apex, and aggravated by an attack of bronchitis. On the 2nd of September I was called to see him, and found the following symptoms :—Considerable prostration ; pulse 100, moderately strong; cough ; occasionally febrile attacks; great sweating at night; intense irritability of stomach, everything being vomited ; the slightest attempt to cough produced vomiting; considerable pain in the back between the shoulders, but more especially towards the left side of the spine, and some pain along the margin of the ribs on the right side; bowels irregular, but at this date rather confined, and discharges of a clay colour; generally lies on the left side. A physical examination of the chest showed on the left side complete dulness from apex to base, increased vocal fremitus, respiration inaudible except a very slight vesicular sound in front, below the clavicle. In right lung the respiration was puerile, mixed with some bronchitic sounds, and prolonged expiratory murmur, preternaturally clear on percussion. The heart sounds were healthy, but weak. (I should here mention that the patient was an inveterate smoker.) Nothing abnormal could be felt in the abdomen, excepting that the edge of the liver was a little below the margin of the ribs. This condition, had, however, been noticed during his attack of fever twenty years before, and had been present ever since.

The diagnosis I arrived at, after due consideration, was cirrhosis of the left lung.

As to treatment, attention was at first directed to the irritable stomach. A small dose of blue pill and a prussic acid mixture were administered, lime water and milk were ordered as a drink, and a small blister dressed with morphia was applied to the epigastrium. Considerable improvement followed this treatment.

On September the 6th I met an eminent physician in consultation, who found pretty much the same class of symptoms I have just detailed, but less intense than on the 2nd of the month.

After examining the patient, this gentleman pronounced the case to be one "of a very grave nature, and that the patient would die of it," but declined to give a more detailed opinion until he had seen or heard more of the case. From time to time I reported the progress of the case, and after about a month had elapsed this physician stated that he considered the case to be one of cancer of the lung and stomach, and that the patient would *die hard.* The treatment I have just detailed was pursued, with but slight alteration, for about a fortnight, at the end of which time the patient was so much improved that he was able to get up, sit in an adjoining room, and employ himself for several hours a day calculating statistical tables for the Government, but still any exertion produced great weakness. He still complained of pain in his back, which was slightly relieved by counter-irritation. This improvement

continued for some little time, and he got so well that he walked round his garden, drove out, and transacted some business matters in town. During the second week in October he got hoarse, and for one day nearly lost his voice. A laryngoscopic examination showed some thickening and redness of the mucous membrane of the larynx. This hoarseness gradually went off, and was followed by pain in swallowing, described by the patient as very low down, and amounting to but a slight soreness. This was in the last week of October. From this time all improvement ceased; from spending several hours a day up, he was gradually out of bed for shorter and shorter periods until the beginning of November, from which time he might be said to have been completely confined to bed. His great complaint was now intense pain in the back and in the left hypochondrium, frequent attacks of vomiting, difficulty of swallowing any but the softest solids. The patient now subsisted entirely on egg flip, milk with lime water, and beef-tea.

On the 23rd of this month I met in consultation another eminent physician, who, having examined the patient, pronounced him to be labouring under aneurism of the aorta pressing on the left bronchus and œsophagus, announcing his belief that the patient would die suddenly, and recommending that any important business or family matters should be arranged as quickly as possible. The patient gradually got worse. He became more emaciated and weaker, his difficulty of swallowing increased, the paroxysms of pain became more frequent and intense, retching was constant, but nothing was vomited but a little mucous, tinged in a few instances with blood. No sedatives had *much* influence; he seldom obtained sleep for more than an hour at a time, and then only by large doses of sedatives, the most useful of these being opium; subcutaneous injections of morphia up to a grain and a half nearly failed to give relief. The greatest relief was obtained by the almost constant inhalation of chloroform, and chloroform applied externally, with or without morphia, gave some relief; this state of things continued gradually getting worse, until the 26th of December, when he became delirious, apparently suffering much pain. He died the following day. I must mention that for the last six weeks of his life the patient could not lie on the left side, owing to the intense dragging pain he felt when in that position. He described the pain as if his liver were being dragged from the right towards the left side.

With consent and wish of the patient's friends, a *post mortem* examination was made 60 hours after death. The body was greatly emaciated, there being scarcely a trace of fat. On opening the chest, the left lung was apparently absent. Some fluid was found in the left pleural cavity, and a tumour somewhat larger than a shut fist was found occupying the position of the root of the left lung; the outer and anterior part of this tumour was composed of carnified lung, the posterior and internal part,

which was adherent to the spine, filled up the arch of the aorta, and passed insensibly into the carnified lung. It consisted of a white brain-like mass, the consistence being uniform and somewhat harder than brain. This mass enclosed and completely obliterated the left bronchus. The right lung was somewhat emphysematous; the heart was of natural size, paler and softer than natural; the aorta perfectly healthy, and its calibre not larger than natural.

On removing the heart and lungs several small masses of encephaloid matter similar to that involving the root of the lung was found lying along the œsophagus. The lower end of the œsophagus was found to be involved in a mass of this cerebriform substance, the mass extending down behind, and involving the superior and posterior part of the stomach. On opening the stomach it was found to contain some coffee-ground matter, but no ulcer could be seen. The little finger could scarcely be passed into the œsophagus. The disease involved a few mesenteric glands, and a small portion of the large end of the pancreas. The liver was somewhat larger, and its anterior edge harder than natural, but no deposit similar to that found in the chest and stomach was discernible in this organ.

Microscopical examination demonstrated the tumours to consist of a fine granular matter; no cells could be discovered in any, even the hardest parts. My friend Dr. Robert M'Donnell also examined this substance, and agrees with me in considering it to be cerebriform cancer.

As I stated at the commencement of these remarks, the chief interest of the case lies in the difficulty of diagnosis, and it is not a little humiliating to find such a difference of opinion concerning a case where *post mortem* examination showed such well marked and extensive disease. There were four well marked opinions concerning the disease under which this patient laboured.

1st. Tubercle of the lung.—This opinion was held by an eminent physician of this city, and by two other professional men who have had large experience, and whose opinions are respected.

2nd. Cirrhosis of the lung.—This was my own opinion, and in it I had the concurrence of an able and acute medical officer of her Majesty's navy, who had the same opportunities of observation as I had.

3rd. Cancer of the lung.—Pronounced by an eminent physician and distinguished member of this Society.

4th. Aneurism of the aorta.—By another eminent and distinguished member of this association.

Several questions at once occur to us, on learning the details of such a case. How could such a variety of opinions prevail? and how can this case lead to more certain diagnosis in future and similar instances? The question of tubercle I set aside on the grounds of no family history, too little respiration (in fact it was almost, if not altogether, absent in

the affected side), no phthisical expectoration of any kind. I believe the opinion was formed on the grounds of something very closely allied to, if not identical with, hectic fever, general debility, some doubtful crepitation under left clavicle, dyspepsia, retching, and irregular bowels. I believe there were fair *rational* grounds for this opinion.

The opinion of cirrhosis of the lung was formed upon the extensive absence of respiration from the left side; absence of any of the expectorations indicative of phthisis, with many of the rational signs of that disease present.

The diagnosis of cancer was grounded more, I believe, upon the method of exclusion than upon the character of the physical signs, and by the general look of the patient (which, I must confess, did not seem to me sufficiently malignant to attract my notice until it was recognized and remarked upon by my more experienced senior); by the intense paroxysms of pain, and soreness of swallowing. It was this soreness afterwards becoming an obstinate and painful obstruction, that ultimately led me to agree that the disease was of a malignant nature, and had involved other organs than the lung.

The opinion, aneurism of the aorta, was chiefly founded upon the detection of two centres of pulsation in the chest, together with a doubtful aortic murmur. My reason for not concurring in this opinion was, that I believed an aneurism of the aorta of sufficient magnitude to completely close the left bronchus, and produce such obstruction to swallowing, would produce some, or probably a considerable number, of those head, neck, and such symptoms with which we are familiar in cases of thoracic aneurism. The *post mortem* examination, however, showed what fair grounds there were for this belief in thoracic aneurism. The tumour involving the root of the left lung almost closed up the arch of the aorta, and encircled to some extent its descending portion, gluing together all the adjacent parts, thus forming a mass which must necessarily be moved by each pulsation of the aorta, and perhaps causing sufficient pressure upon the great vessels to produce a murmur. I did detect this pulsation, but never the murmur; the pulsation often seemed insignificant, and, taken with other symptoms, insufficient to denote aneurism.

In a case of this sort it is impossible to fix, with any degree of accuracy, the first locality attacked by the disease. In this case I believe, however, it commenced in the glands near the root of the lung, and must therefore be considered to be more of the nature of mediastinal than pulmonary tumours, and we know the difficulties attending the diagnosis of mediastinal tumours.

I am afraid the question of how to diagnose a similar case cannot be definitely assumed; nevertheless, I think this case may be *suggestive* as to the determining the diagnosis of obscure diseases within the thorax, and it is with this hope that I have laid it before the Society.

On General Dermatitis, or Pityriasis Rubra. By J. Hawtrey Benson, M.B., L.K.Q.C.P.I.; and Walter G. Smith, M.B., F.K.Q.C.P.I.

The following is a description of a case of an extremely rare form of skin disease. It is an example of general superficial inflammation of the skin, or, according to Hebra's nomenclature, *Pityriasis Rubra.* It is the same which Erasmus Wilson, in his last edition, terms *Pityriasis Rubra Foliacea.*

As a proof of its rarity, we may mention that up to the time the second volume of the English translation of Hebra's work on *Diseases of the Skin* was published, namely, in 1868, that great dermatologist had seen but *three cases.* When Erasmus Wilson published his work in 1867, he had met with but *two cases.* Since that time, however, Mr. Wilson has published a third case of the disease in the *Medical Times and Gazette* for January 29th of this year, and there he calls it *Dermatitis Exfoliativa.* In *Guy's Hospital Reports* for the year 1861, Dr. Wilks published a case which occurred in the practice of Dr. Owen Rees, under the name of "General Inflammation of the Skin, or Dermatitis," which appears to correspond with the *Pityriasis Rubra* of Hebra. Devergie, too, has met the disease. But, with these four exceptions, we are not aware of any other author having described it. The first writer who mentioned the name of *Pityriasis Rubra* was Bateman, in his continuation of Willan's work, *On Cutaneous Diseases*, but he gave no definition of it applicable to all cases of this affection, and which would exclude other complaints resembling it. In fact, Hebra concludes that Bateman never saw the disease in its general form; and that, in like manner, a number of others who have since written on the subject of skin diseases, including Biett, Green, Cazenave, Schedel, Chausit, Gibert, Frank, Alibert, Fuchs, Duchesne-Duparc, and Rayer, had never seen the disease in the *general* form which he himself describes, and of which we are about to give the notes of a case.

T. F., aged forty, a labourer, employed in chemical works, applied for relief at the Adelaide Hospital Dispensary, on October 4th, 1869. Six weeks previously, without any premonitory symptoms, he noticed an eruption of "little red spots" on the trunk and abdomen, which spread rapidly over the whole body. The spots soon became confluent, producing a continuous red surface. This condition was followed, in a few days, by general exfoliation of the epidermis in *large* whitish flakes, which averaged from about the size of a threepenny piece to that of a shilling. The entire surface of the body, from the head to the feet, was of a vivid deep-red colour, which partially disappeared on pressure, leaving a yellowish impression.

On this background the epidermic scales were seen disposed parallel to the lines of flexion. They stood out large, lustrous, very thin—like silver paper—exceedingly abundant, and were easily rubbed off. One edge,

generally the inferior, was free, and was raised a line or two above the level of the skin underneath, while at the opposite border the scale was continuous, with the deep red cuticle as yet undetached. He had a sensation as if the skin was dry and parched, and he felt unwilling to bend his joints lest the skin should "crack," as he expressed it. The thick cuticle of the palms of the hands freely peeled off. Of the true skin there was but little infiltration; no considerable exudation into it, nor on its surface. There was no itching except at night (and even then it was inconsiderable), nor any other unpleasant sensation, except some numbness at the lower part of the abdomen. The conjunctivæ were slightly injected. He never suffered from any form of venereal disease, and was always a healthy man till this eruption appeared. Since that time he had slightly emaciated, and had become so weak that he was confined for the most part to bed. He constantly shivered, and was very susceptible of cold. The pulse was over 100, and compressible; and he complained of nocturnal palpitations. His appetite was not affected. He was ordered a grain of quinine four times a day.

On the 7th he was no better, and as he was not admissible into the Adelaide Hospital, Dr. Smith placed him under the care of Dr. Hawtrey Benson in the City of Dublin Hospital. After a few days rest in bed, the pulse had fallen to 70, and the temperature in the axilla varied from day to day between 99·2 and 99·6. The tongue was abnormally red, especially at the margin and tip. It was partly covered with a thin white fur, which was deficient in irregular patches, disclosing the bright red surface of the tongue underneath. His bowels were regular. The urine was slightly acid, rather scanty and high coloured, and contained an abundance of phosphates, but no albumen nor sugar. But for the redness of the tongue and slight injection of the conjunctivæ, the mucous tracts seemed unaffected by the disease.

The epidermic scales from the body were shed in very great quantities, and reproduced with a rapidity quite remarkable. Every afternoon most of the loose scales were rubbed off in the bath which he took daily, and yet by next morning the scales lying in his night-shirt and on his under-sheet would, at least, fill a four or six ounce measure glass. The nails of both fingers and toes were unaffected throughout. No rhagades appeared in any part. In about a week after his admission he suffered from two superficial abscesses in each axilla, from which he soon recovered.

He was ordered warm, alkaline baths daily, and the application of emollient ointments. These latter were most grateful to him, and he continued the use of the baths and ointments from the commencement to the termination of the treatment. Of internal remedies we at first employed consecutively quinine, iron, the mineral acids, strychnia, and belladonna, but without in the least modifying the disease. On th

contrary, the debility steadily progressed. Disappointed at finding no improvement after about a fortnight's treatment, the patient left the house upon some trivial excuse. However, after a week's enjoyment of home society with starvation and filth, he presented himself again at hospital, considerably worse than when he left it, and was readmitted. While he was out the thick cuticle of the soles of the feet desquamated in large sandal-like flakes.

When he returned we put him immediately upon arsenic, using alternately the liquor arsenicalis (Fowler's) and the liquor arsenici hydrochlorieus (de Valangin's).

In a few days afterwards the scales became less numerous and smaller. Next, the skin underneath gradually lost its intense redness; his general health improved, and in less than a month from his readmission, that is, in about seven weeks from the commencement of treatment, or thirteen from the first appearance of the disease, almost every trace of desquamation had disappeared, except about the head; and the skin had almost quite regained its natural colour and pliancy. Scattered over the trunk were left a few brownish-red patches of pigment not altered by pressure, irregularly oval and about the size of a sixpenny piece. These patches were longer surmounted by scales than the neighbouring skin.

The first thing that strikes us in this case is the very slight constitutional disturbance attending such very considerable lesion of the *entire* cutaneous surface. The patient ate and slept well, and was free from pain or unpleasant sensation, with the exception of slight pruritus at night. In fact, the only considerable constitutional symptom was debility, joined with slight increase of temperature, and slight acceleration of pulse.

As to diagnosis, both Hebra and Wilson enter at some length into the question of the differential diagnosis between this disease and *eczema*; but the boundary-line seems pretty well defined. The absence of moist exudation, and of distressing pruritus; the characters of the epidermic lamellæ—large, thin, profuse, and exhibiting no trace of corpuscular matter—as well as the universality of the disease over the whole body, no external part escaping, are sufficient to exclude *eczema* in any of its stages. Devergie, when speaking of *pityriasis rubra*, lays it down that no other disease is ever universal over the whole cutaneous surface, except *acute lepra*.

As a full consideration of the points of differential diagnosis between *pityriasis rubra* and other diseases likely to be confounded with it, would be altogether too tedious, we will merely quote a few lines from Hebra bearing on the question. "As appears from its name," that author says, "the affection which is termed *pityriasis rubra* consists in a reddening of the skin, which is also covered with scales. But, as is well known, these characters are likewise found in other dermatoses, such as *psoriasis*, *lichen*,

eczema, and *lupus erythematosus;* and therefore we have to give some additional symptoms belonging only to the disease in question, and absent in those which thus resemble it. In doing this, however, we must rely not so much on positive as on negative characters. In the above-named affections, besides the redness and the scales, there are always other symptoms. Either there is infiltration of the cutis, with or without the presence of fissures; or there is oozing of a fluid secretion; or there is severe itching, which leads inevitably to scratching, and consequently to the existence of excoriation; or, lastly, the disease occupies some particular region, or presents certain peculiarities in its course. But in the *pityriasis rubra* all these are absent. In it there is nothing more than an intense redness diffused over a large part of the skin, or even universal, disappearing beneath the pressure of the finger (when it gives place to a yellowish colouration), and accompanied by the presence of fine white loosely-adherent scales, which result from the constant shedding of the most superficial layer of the cuticle."

According to Hebra's experience cases of this disease are very hopeless. Each of his three cases ended fatally, after years of duration. The disease was apparently uninfluenced in the least by any internal remedies. Arsenic, tincture of cantharides, sulphur, antimony, decoctum lignorum, sarsaparilla, iodine, and mercury, all failed alike to produce any change whatsoever in the characters of the disease. However, by the repeated employment of tepid baths, and by the application of oils and emollient ointments, the disease "has, to a certain extent, been modified" under his observation. But in these countries the disease does not seem so hopeless. One of Wilson's three cases ended in recovery; another ended fatally through the effects of intercurrent bronchitis; and the third was still labouring under the disease at the time he published the account of it in the *Medical Times and Gazette*. Dr. Owen Rees' case ended favourably. Our case also recovered completely, and we are inclined to give a very large share of the credit, if not the entire, to the use of arsenic.

Report of Cases of Scarlatina followed by unusual Sequels. By S. M. MacSwiney, M.D., Physician to Jervis-street Hospital.

Case I.—On 11th December, 1869, I was asked to attend Monsieur Ade V——, a Belgian young gentleman, aged eighteen years, for an attack of fever. At my first visit I learned that he had been complaining of illness for the previous twenty-four hours; and that he had had headache, sick stomach, and shiverings. I found him in the following condition:— His pulse was 120 in a minute; his tongue was densely coated white; he had thirst, and was restless and indisposed for sleep. He complained of sore throat, and upon inspection, I found the tonsils much swollen,

and their surface, as well as that of the entire pharynx and mouth, was of a deep scarlet colour. There were no ulcerations in any part of the mucous membrane. Upon examining the skin of the body, I ascertained the existence of several large sized patches of the characteristic scarlet eruption over different parts of the trunk. It was punctiform, bright red in colour, diffused. The separate patches coalesced next day, and covered the entire trunk of the body. The diagnosis was easy enough; he had a mild attack of scarlatina, and went through it without any unusual symptom until the tenth day. His pulse had fallen to 80; his tongue had quite cleaned; the eruption had entirely disappeared; the urine was clear, and non-albuminous—in a word, he seemed to be quite convalescent. At this time, however, namely, the tenth day after the appearance of the eruption, his stomach became sickish; his pulse suddenly rose to 120 again; he spent a sleepless night, and was thirsty. The urine he passed was dark, almost black indeed, in colour; and submitted to tests, it was found not to contain blood; his conjunctivæ were tinged with yellow, and there was an icteric hue over the skin of the body generally. On the next day he had well-marked jaundice. This showed all the usual symptoms of an ordinary attack of that morbid state, and lasted for about eight days, after which time all the symptoms subsided; he made a rapid and good recovery, and has remained in excellent health up to the present time.

Case II.—On 1st January of this present year, I was asked to see, in consultation, two young ladies, sisters, at Booterstown. I found that they were patients of my friend Dr. R. Griffin, who had been for some days in attendance, and, recognizing the nature and gravity of their state, had now called for professional assistance. I was thus associated in attendance with him, and continued to visit the invalids for one month, when I left them convalescent. *History.*—Dr. Griffin informed me that the younger sister, A——, had been attacked with shivering, intense headache, and fever, five days before my first visit. When he saw her two days after the commencement of her illness, he was struck with her extremely weak state. His attention having been directed to her throat, he found it greatly swollen and inflamed; her tongue was also much swollen and coated; ulcerations prevailed extensively in the mouth and throat; there was, at the same time, a good deal of general fever. Suspecting at once the nature of the case, Dr. Griffin made a most particular search for the scarlatinal rash, but failed to observe a trace of redness; and was assured, moreover, that there had not been any eruption on any part of the body previous to his visit. On the occasion of my first visit, the patient A—— presented all the appearance of the most alarming depression; her face had a pale, wasted, and pinched look; her pulse was very rapid; could scarcely be counted indeed, and was

extremely feeble; she spoke with great difficulty, in consequence of the inflamed state of her tongue, mouth, and pharynx; her voice was indistinct and feeble; she could not open her mouth more than half an inch wide; the skin of the neck was dusky-looking, and it and the underlying tissues were greatly swollen. Inspecting the mouth as well as I could, I made out that the tongue, hard and soft palate, and the cheeks internally, were dry, glazed, and covered with a tough dark-brown crust. The surface of the tonsils and adjacent parts was in a greatly disorganized, ulcerated, yellowish-grey sloughy state. There was *sordes* on the teeth and lips; and she was so weak, that the assuming of the semi-erect position for the purpose of being examined, very nearly caused syncope. Her thirst was extreme. The history of the case, the symptoms which had ushered in the sickness, and the nature and appearance of the lesion of the throat, enabled me to fully confirm Dr. Griffin's diagnosis of "scarlatina maligna sine eruptione." Notwithstanding that the length of time which had elapsed since the invasion of the disease was sufficient to admit of the disappearance of any cutaneous eruption, if such had ever been present, I still thought it my duty to personally investigate this point; I therefore repeated Dr. Griffin's search for a rash, and with a like negative result. I was also, as he had been, informed by the nurse, that she had previously looked all over the body, and that there had not been an eruption at any period of her illness. With the utmost difficulty the patient struggled through this severe attack, being, indeed, at times, in an almost hopeless condition, until the twelfth day of her illness, when being generally better than she had, as yet, previously been, two noteworthy observations were made, viz., (*a*) the skin at the root of the nails of both hands began to crack and fall off; and (*b*) severe pains, which we looked upon as being rheumatic in character, were felt by the patient in her shoulder, and other joints. The process of desquamation, which we have seen, was thus commenced at the ends of the fingers, went on increasing until it involved the epidermis of the entire body, large flakes coming off in some parts. The rheumatic pains yielded to treatment; but on the fifteenth day of her sickness, the patient complained of pain in the groin and left leg, with inability to move the limb.

Upon examination of this, we ascertained the following to be the state of affairs. The thigh and leg were semiflexed and somewhat swelled. The skin over these parts was white, bloodless, and shining. There was pain and thickening in the course of the femoral and crural vessels. Pressure on the back of the calf of the leg, in the centre, could not be borne, whilst pressure in the course of the femoral vein was most unwillingly permitted to be made. In each instance, the knotted, cord-like feel of the thickened vessel could be easily made out. In fact, the vein, and its branches where they could be felt, were completely closed by coagulum or adherent fibrine. There was, as I have said, pain complained of; and

this was, distinctly, of two different kinds, viz. (1), a dull pain, which was constantly present, and (2), a violent pain, which was intermittent, and caused the patient to scream out whenever it recurred. The affection progressed in severity day by day, the vein of the right leg also soon became engaged in the same way; the swelling was now enormous, there was great œdema, and the pain aggravated by the slightest movement of the limbs, was, even during periods of perfect rest, intense; large superficial veins, full of dark blood, made their appearance on the thighs, abdomen, and pelvis, and carried on the collateral venous circulation. Her shoulders and hips suffered from the decubitus which was constantly on her back; she became haggard; sordes continued to form upon her lips and teeth; she had the "facies hippocratica;" her pulse was scarcely perceptible; and we greatly feared she would never survive through this almost hopeless state. However, just as matters appeared to be at the worst, she began to mend, and ultimately, on the 32nd day of her illness, was convalescent.

Case III.—S., sister to the patient whose case has just been related, was attacked two days later than A., with headache, thirst, fever, and sore-throat. She had no "rash" on her body, a fact which was ascertained with certainty. She had always been a weak, delicate girl, and from the outset of her illness, she was greatly prostrated. Her pulse was quick and extremely weak; she could not sleep, and was unable from total disrelish of food, as well as from the soreness of her mouth and throat, to take much nourishing drinks. She swallowed, indeed, little else save iced water, barely coloured with claret, and small quantities of milk. Her tongue was dry, brown, and rough in the centre; white and moist at the edges. She slowly progressed, in an unsatisfactory way through her illness, when, on the eighth day symptoms of *typhoid fever* presented themselves. She had tympanitis; she complained of much pain in the ileocecal region (which was tumid from air), and when pressure was made in that situation, gurgling was distinctly elicited. Her bowels were loose, being opened three or four times in the twenty-four hours. There were some eight or ten rose coloured spots scattered over the surface of the chest and abdomen. Her weakness and prostration were extreme. At no time were there any symptoms of disturbance of the intelligence. The typhoid fever now became the prominent disease to be attended to; she passed through it moderately well, the chief danger being from weakness arising from her inability to take any sufficient amount of suitable nourishment. It was very interesting to watch, as we did, the desquamation of the epidermis at the ends of the fingers, where the matrix of the nails is inserted, due to the scarlatina, going on at the same time that the patient was passing through the stages of a somewhat mild attack of undoubted typhoid fever. Ultimately, desquamation

having become general over the body, she recovered strength, and became entirely convalescent. I will now sum up, in a few words, the facts which I deem of interest in these cases. I think the rarity of the *sequels* to scarlatina met with by me in the three patients, whose histories I have just recorded, will be conceded :—

1st.—In case No. 1, jaundice supervened. This affection does not often, undoubtedly, occur as a sequel to the disease, and when it does occur must be due to one of three causes, viz. :—(*a*) Congestion of the liver. (*b*) Catarrh of the bile ducts. (*c*) Blood poisoning. Dr. George Harley (*Pathology and Treatment of Jaundice*, p. 93), relates a case of scarlatina in which jaundice occurred. Dr. Graves, in his *Clinical Lectures*, Vol. i., p. 453, mentions two cases of scarlatina in which jaundice was a complication. Murchison (*Lectures on Diseases of the Liver*, p. 388), observed this sequel of scarlatina in five instances. Three out of these five cases had a fatal termination. Other observers, also, have met with the affection in this class of cases, but it is, confessedly, an unfrequent occurrence.

2nd.—In case 2, phlebitis of a very severe nature occurred. I have not met with any record of this serious complication of scarlet fever having been met with before. Sir William Jenner, indeed (Gulstonian Lectures, 1853), pointed out the occurrence of a species of pyemia found to follow scarlet fever and other acute affections. And indeed we had felt, in the course of this disease, that it was quite possible, and even not unlikely that pyemia might supervene. But I have no where heard or read of *phlebitis* after scarlatina.

3rd.—I make the same remark respecting case three where scarlatina was followed by typhoid fever. This complication does not appear to have been observed previously. The nearest approach to a recognition of the relation of these two diseases to each other, as met with by me in this instance, is made by the late Mr. Hillier, who, at page 308 of his book on *Diseases of Children*, says "scarlet fever in the course of typhoid fever has been noticed in a number of cases." But here the scarlet fever is the complication ; whilst in my case it was the primary disease.

4th.—Finally, the two cases last detailed may be fairly set down as instances of scarlatina without the scarlet rash. Sir W. Jenner, indeed, doubts if this state is ever really met with, and suggests that in the supposed instances of it which have been recorded, the rash was slight and evanescent, and had thus escaped observation. But in reply to this it is to be remembered that many experienced physicians have testified to the correctness of the statement of its occasional occurrence. I will name but one, the illustrious *Trousseau*, who teaches that cases of scarlatina without rash are often met with.

It was, to our minds, quite satisfactorily made out that there had not, at any time during the sickness, been an eruption in the case of the two

sisters, in both of whom, however, extensive desquamation followed in due course. And Dr. Graves, who maintains the reality of scarlatina without an eruption, gives cases which conclusively establish the fact of its sometimes occurring. It was on account of the several points thus briefly enumerated that I thought it would be interesting to the members of the Medical Society to hear the detail of these cases.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

THIRTY-FIRST ANNUAL SESSION.

Dr. Ringland, President.

On the Muscular Forces employed in Parturition, their Amount and Mode of Application. By the Rev. Samuel Haughton, M.D., Dubl., D.C.L. Oxon.; Fellow of Trinity College, Dublin.

In the first stage of natural labour, the involuntary muscles of the uterus contract upon the fluid contents of this organ, and possess sufficient force to dilate the mouth of the womb, and generally to rupture the membranes. I shall endeavour to show that the uterine muscles are sufficient, and not much more than sufficient, to complete the first stage of labour, and that they do not possess an amount of force adequate to rupture, in any case, the uterine wall itself. In the second stage of labour, the irritation of the fetal head upon the wall of the vagina, provokes the reflex action of the voluntary abdominal muscles, which aid powerfully the uterine muscles to complete the second stage, by expelling the fetus. The amount of available additional force given out by the abdominal muscles admits of calculation, and will be found much greater than the force produced by the involuntary contractions of the womb itself.

The mechanical problem to be solved for both cases is one of much interest and some difficulty, as it is the celebrated problem of the equilibrium of a flexible membrane subjected to the action of given forces. It has been solved by Lagrange (*Mecanique Analytique*, p. 147). In the most general case of the problem, the following beautiful theorem can be demonstrated. Let T denote the tensile strain acting in the tangential plane of the membrane, applied to rupture a band of the membrane one inch broad; let P denote the pressure resulting from all the forces in action, perpendicular to the surface of the membrane, and acting on a surface of one square inch; and let ρ_1 and ρ_2 denote the two radii of principal curvature of the membrane, at the point considered. Then we have the following equation—

$$P = T \times \left(\frac{1}{\rho_1} + \frac{1}{\rho_2} \right) \qquad (1)$$

If the surface, or a portion of it, become spherical, the two principal curvatures become equal, and the equation becomes—

$$P = \frac{2T}{\rho} \qquad (2)$$

In the case of the uterus and its membranes, the force P arises from hydrostatical pressure only, and is therefore easily measured, and the supposition of spherical curvature is approximately admissible.

In the first stage of labour, the contraction of the muscular walls of the uterus compresses its liquid contents, and the membranes are pushed through the mouth of the womb, bulging out into the segment of a sphere, and are in most cases ruptured by the pressure of the liquor amnii contained in them.

It is evident from the form of the gravid uterus that its curvature is greatest near its mouth, and equation (2) shows that for a given hydrostatical pressure, the tensile strain is proportional to the radius of curvature; hence this strain will be greatest at the fundus of the uterus. If we assume the shape of the uterus to be that of a prolate ellipsoid, whose longer diameter is 12 inches, and shorter diameter 8 inches, its mean curvature will be that of a sphere whose diameter is 9·158 inches.

The volume of the gravid uterus is found from the expression—

$$\text{Volume} = \frac{4}{3}\pi ab^2; \qquad (3)$$

in which a and b are the semiaxes, and π is the ratio of the circumference of a circle to its diameter; substituting for a and b their numerical values, we find the contents of the uterus to be 402·13 cubic inches.

The surface of the gravid uterus may be found from the equation—

$$\text{Surface} = \frac{2\pi ab}{e}\left(\text{Sin}^{-1}e + e\sqrt{1 - e^2}\right); \qquad (4)$$

in which e is the eccentricity of the generating ellipse. If the numerical values be substituted in this expression, it will be found that the surface of the uterus is 270·66 square inches.[a]

Some highly interesting conclusions may be drawn from the preceding calculations, combined with the weight of the total muscular tissue of the uterus. Heschl estimates the weight of the uterine muscles at from 1 lb. to 1·5 lb. Montgomery found the muscles of the gravid uterus to weigh 1·5 lbs.; and Levret estimates them at 51 cubic inches, which, with a

[a] Levret estimates the contents of the gravid uterus at 407 cub. in., and its surface at 339 sq. inches. Poppel estimates the contents at 300 cub. in., and the surface at 210 sq. inches.

specific gravity of 1·052, I find to be equivalent to 1·93 lb. Taking the mean of these estimates, we have--

Weight of Muscular Fibres of Gravid Womb.

Heschl,	- - -	1·25 lb.
Montgomery,	- - -	1·50 „
Levret,	- - -	1·93 „
Mean,	- -	**1·56** lb.

If we now suppose this quantity of muscle to be spread over the entire surface of the uterus, we find—

$$\text{Mean thickness of muscular wall of uterus,} = \frac{1{\cdot}56 \times 7000 \times 1000}{252{\cdot}5 \times 270{\cdot}66 \times 1052} = 0{\cdot}1519 \text{ inch.}$$

If we suppose a ribbon, one inch in width, to be formed from the wall of the uterus, its thickness will be 0·1519 inch ; and as each square inch of cross section of muscular fibre is capable of lifting 102·55 lbs., we find for the greatest tensile force produced by the contraction of the uterine muscles.

$$\text{Tensile strain of uterine wall per inch} \} 102{\cdot}55 \times 0{\cdot}1519 = 15{\cdot}577 \text{ lbs.}$$

Substituting this value of T, in the equation (2)—

$$P = \frac{2T}{\rho}$$

and for ρ, its mean value 9·158 in., we obtain the maximum hydrostatical pressure inside the gravid uterus producible by the contraction of its muscular fibres.

$$\text{Maximum hydrostatical pressure produced by uterine contraction} = \frac{2 \times 15{\cdot}577}{9{\cdot}158} = \mathbf{3{\cdot}402} \text{ lbs.}$$

One hundred experiments were made by Duncan and Tait, upon the hydrostatical pressure necessary to rupture the membranes which contain the liquor amnii, which are recorded in Dr. Duncan's book[b] (p. 306–311.) The greatest pressure observed was 3·10 lbs., and the least was 0·26 lb. ; and I find that the mean rupturing pressure of all their experiments was **1·2048** lbs.

Combining this experimental result with the calculation already given, of the amount of pressure producible by the muscular tissue of the womb, we may conclude that the uterine muscles are capable of rupturing the membranes in every case, and possess, in general, nearly three times the amount of force requisite for this purpose.

The weight of muscle in the virgin uterus is about one-and-a-half ounces, and this muscle is increased to one-and-a-half pounds during pregnancy ; that is, it is increased about sixteenfold. If ever there was a muscular system produced to effect a specific object, the uterine muscle

[b] Researches in Obstetrics. Edinburgh, 1868.

may be regarded as such; and if the principle of least action in nature be valid, we should expect such a muscle to be developed only to the extent required, and not beyond it. Considered from this point of view, the preceding calculation must be regarded as interesting, for it shows that the uterine muscle possesses a force of 3·4 lbs., intended to overcome a maximum resistance of 3·1 lbs. It would be a waste of power to endow the uterus with more force than I have shown it to possess, for it is not necessary that the uterus should complete the second stage of labour, as the abdominal muscles are available for this purpose; so that by using them, and not giving the uterus more force than is absolutely necessary for the first stage of labour, an admirable economy of muscular power is effected.

Viewed in connexion with the doctrine of final causes, the uterine muscle presents a subject well worthy of contemplation. It is plainly necessary that the first stage in the expulsion of the fetus should not be entrusted to a voluntary muscle, and hence an involuntary muscle is gradually provided, which takes the initiative and commences the process of parturition, the completion of which is then accomplished by the aid of voluntary muscles, to the employment of which, at this stage, no moral objection can be raised. It is also necessary (if the Contriver be allwise, or if the principle of least action in nature be true), that the involuntary muscle, so produced, should not possess more or less force than is requisite for its purpose. The uterine muscle does not grow, to meet a growing resistance (as happens frequently in other cases), and its precise degree of strength cannot be produced by a tentative process; for in healthy gestation, the uterine muscle never tries its force against the membranes it is called upon to rupture until the actual period of parturition has arrived. We witness, therefore, the instructive spectacle of a growing force tended to overcome, at a definite future period, a definite resistance, against which it never measures its strength until the actual moment of conflict has arrived.

The mind must be singularly constituted which fails to see in such an arrangement the clear evidence of a Divine Contriver, adapting means to an end foreseen by Himself.

The extreme force of uterine contraction produces a pressure of 3·402 lbs. per square inch, which is equivalent to a pressure of 54·106 lbs. acting upon a circle of 4½ inches in diameter, which is assumed as the average area of the pelvic canal.

The maximum force used to expel the fetus, by both uterine and abdominal muscles combined, is estimated by Joulin,[a] by forceps experiments made on the dead body, at 110·23 lbs., a result which is regarded by Dr. Duncan[b] as too large. Dr. Duncan himself considers 80 lbs. as

[a] Traité Complet des Accouchements, p. 447.

[b] Researches in Obstetrics, p. 323.

the maximum force ever employed in difficult cases. This would correspond with an hydrostatical pressure inside the uterus of 5·03 lbs. per square inch, which is greater than the uterine muscles, unaided, are capable of producing.

Dr. Duncan's experiments on the strength of the uterine wall itself show that an hydrostatical pressure of from 17 to 25 lbs. per square inch is necessary to its rupture; a result which conclusively demonstrates, when taken in conjunction with the preceding calculations, that the uterus can never destroy itself by its own contraction. It would, indeed, be *a priori*, ridiculous to suppose that Nature would produce an involuntary muscle capable of destroying itself, and so causing the death of its owner by its own blind force.

In concluding my remarks on the uterine muscles, I may observe that the arrangement of these muscles on various parts of the organ, affords a confirmation of the accuracy of Lagrange's principle as applied to them. It is well known that these muscles attain their greatest thickness at the fundus of the womb, where they are fully double the thickness of the muscular layer at the neck of the womb. Now, so long as the contents of the womb are fluid, or semi-fluid, a constant pressure is exerted perpendicularly to all portions of its walls, requiring, by Lagrange's theorem, a tensile muscular force varying as the diameter of curvature at each point. From observations on the curvature of the gravid uterus, it may be inferred that the diameters of curvature at the fundus and neck of the womb, are nearly in the proportion of 29 to 17. When the womb is emptied of its contents, and the uniform hydrostatical pressure inside is removed, the uterine walls contract with degrees of force proportional to the diameters of curvature they possessed in their former condition; the fundus contracting with twice the force of the parts near the neck, an action eminently calculated to promote the safe and speedy reduction of the organ to its diminished bulk, without risk of inversion.

It is worth while to consider for a moment, the explanation offered by the Lamarckian theory of natural selection, of the relation between the strength of the uterine muscles and the resistance of the membranes they are required to rupture. According to this theory, an animal that possessed such a proportion as I have shown to exist between the force and resistance to be overcome, must be the descendant from ancestors who attained this proportion accidentally, and transmitted it to their posterity, who thus acquired an advantage over their neighbours, in the competition for life. Thousands and tens of thousands of those who have preceded us, must have perished in the race, from not having had the precise balance between uterine force and resistance, which we happily possess; and we may regard ourselves as the survivors of a contest in which the slain greatly outnumber the victors. According to this school of philosophy, an economy of two per cent. of wax will make a bee; and a

saving of a quarter ounce of uterine tissue may, possibly, constitute the sole difference between a Celtic and a Teutonic woman.

Happy is the man whose mother possessed the hereditary tendency to form no more uterine muscle than was absolutely necessary to push him into the world! Happy is the bee who can form her cell with a saving of two grains of wax in the hundred, for she will distance in the race of life her competitors who have not inherited this precious art!

In the second stage of labour, the voluntary action of the abdominal muscles is called into play, to aid the expulsive efforts of the uterine muscles. I have attempted to calculate the force available from the contraction of these muscles as follows:—

The abdominal muscles are four in number, viz.—*rectus abdominis*, *obliquus externus*, *obliquus internus*, and *transversalis*. The last three muscles form curved sheets, acting upon the corresponding muscles of the opposite side by means of tendinous *aponeuroses* which meet in the linea alba, and form the sheath of the vertical *rectus abdominis* muscle. From the arrangement of all four, it is plain that the tensile force of muscular contraction in the curved wall of the belly from the xiphoid cartilage to the symphysis pubis, is to be measured by the sum of the united forces of all the muscular sheets. If we knew the force of each muscle and the principal curvatures of the belly in the middle line, we could calculate, by Lagrange's theorem, the hydrostatical pressure inside the abdominal cavity, and available to expel feces, urine, or a fetus.

In order to ascertain the force of the muscles, I measured carefully their average thickness in three subjects, of whom one was a young woman who had borne children, and the others were men of ordinary size and appearance. The results obtained were the following:—

Thickness of Abdominal Muscles.

	No. 1—Male	No. 2—Female	No. 3—Male
Rectus abdominis, -	0·275 in.	0·29 in.	0·34 in.
Obliquus externus, -	0·200 „	0·25 „	0·19 „
Obliquus internus, -	0·235 „	0·17 „	0·24 „
Transversalis, - -	0·127 „	0·15 „	0·14 „
Totals, -	0·837 in.	0·86 in.	0·91 in.

The average total thickness of the muscular walls is 0·869 inch, which is nearly identical with the measurement obtained from the female subject. It has been often shown that we must add 50 per cent. to the weights of muscles in the dead subject in order to bring them to the living weights. This correction gives us 1·3035 inches for the mean thickness of the muscles causing tension in the central line of the belly, where the forces of all the muscles come into play together. Multiplying this thickness by 102·55 lbs., or coefficient of muscular contraction, we find

$$T = 1{\cdot}3035 \times 102{\cdot}55 = 133{\cdot}67 \text{ lbs.}$$

This is the tensile strain producible by the contraction of the abdominal muscles along the curved central line of the belly.

It remains now to ascertain the principal curvatures of the abdominal surface, and to use the equation—

$$P = T\left(\frac{1}{\rho_1} + \frac{1}{\rho_2}\right),$$

so as to determine P, the hydrostatical pressure per square inch inside the cavity of the belly, and available, either in whole or in part, for the expulsion of the fetus, during the second stage of labour.

In order to ascertain the curvature of the belly, I made experiments upon three young men, placed lying upon their backs upon the floor, and made them depress and raise the abdominal wall as much as possible. The result was as follows:—Taking a straight line from the upper part of the symphysis pubis to the xiphoid cartilage as the fixed line of comparison, it was found possible to depress the navel one inch below this fixed line, and to raise it two inches above it. When the belly was distended to the utmost, by the action of the abdominal muscles, I measured the longitudinal and transverse curvatures, by measuring the sagittas corresponding to a given length of tangent, with the following result:—

No.	Diameter of Longitudinal Curvature	Diameter of Transverse Curvature
J. G. H. -	22·93 inches.	12·30 inches.
H. O. - -	22·73 ,,	12·80 ,,
S. H. - -	22·52 ,,	12·80 ,,
Mean, -	**22·727** inches.	**12·633** inches.

The curvature of the distended belly, at the navel, is found to be, from the foregoing measurements—

$$\frac{1}{\rho_1}+\frac{1}{\rho_2}=\frac{1}{11{\cdot}3635}+\frac{1}{6{\cdot}3166}=\frac{1}{4{\cdot}0596}$$

Multiplying this curvature into the tension of the abdominal muscles at the navel, already found, viz., 133·67 lbs. per inch, we obtain finally—

$$P=\frac{133{\cdot}67}{4{\cdot}0596}=32{\cdot}926 \text{ lbs. per square inch.}$$

This amount of expulsive force per square inch is available, although not usually employed, to assist the uterus in completing the second stage of labour. If we suppose it applied to a surface of a circle 4½ inches in diameter (the usual width of the pelvic canal), we find that it is equivalent to 523·65 lbs. pressure.

Adding together the combined forces of the voluntary and involuntary muscles, we find—

Involuntary muscles = 54·10 lbs.
Voluntary muscles = 523·65 „

Total, **577·75** lbs. av.

Thus we see that, on an emergency, somewhat more than a quarter of a ton pressure can be brought to bear upon a refractory child that refuses to come into the world in the usual manner.[a]

In order to determine, by actual experiment, the expulsive force of the abdominal muscles, I placed two men, of forty-eight and twenty-one years of age, respectively, lying on a table upon their backs, and put a disk measuring 1·87 inches diameter just over the navel; weights were placed upon this disk, and gradually increased until the extreme limit of weight that could be lifted with safety was reached; this limit was found to be, in both cases, 113 lbs. As the circle, whose diameter is 1·87 inches, has an area of 2·937 square inches, the pressure perpendicular to the abdominal wall produced by the action of the abdominal muscles is—

$$P=\frac{113}{2{\cdot}937}=38{\cdot}47 \text{ lbs. per square inch.}$$

This force is somewhat greater than that calculated from the curvature of the belly and the coefficient of muscular contraction, already determined; viz., 32·93 lbs. per square inch. It should be remembered, however, that in my experiments on the abdominal muscles, I pushed the

[a] The preceding result will, no doubt, remind the curious and well-informed reader of the statement made by Mr. Shandy, on the authority of "Lithopædus Senonensis de partu difficili," that the force of the woman's efforts in strong labour pains is equal, upon an average, to the weight of 470 lbs. avoirdupois, acting perpendicularly upon the vertex of the head of the child.

trial of strength almost to the verge of producing slight *peritonitis*, as both the subjects of my experiment complained of sharp shooting pains in the abdominal walls for twenty-four hours after the experiments were made.

The foregoing theory of the muscular forces employed in parturition, leads us naturally to a caution as to the use of chloroform, and other anesthetics, which produce their effects, first upon the voluntary, and afterwards upon the involuntary muscles. When chloroform is used, in parturition, beyond the stage at which it produces simple drunkenness, and indifference to pain, it is positively injurious; for it destroys the action of the voluntary abdominal muscles, which constitute the chief part of the force employed in difficult labours. Hence, the muscular tissue of the uterus is left to overcome an obstacle beyond its strength, and without the aid that nature intended to be given by the abdominal muscles. The result is that labour ceases until consciousness returns, and enables the ill-treated woman to avail herself of the apparatus of abdominal muscles provided by nature for her use.

In these remarks I confine myself altogether to the physical aspects of the use of chloroform in labour; and forbear to express an opinion as to the moral effects likely to be produced by the exhibition of an intoxicating agent, administered merely for the luxurious purpose of deadening the pain of the exercise of a purely natural physiological function.

Ovarian Tumour, Complicating Labour. By George H. Kidd, F.R.C.S.I.; Obstetric Surgeon to the Coombe Lying-in Hospital, &c.

Ovarian tumours, as complicating labour, divide themselves into two classes, viz.: those lying low in the abdomen and obstructing the passage of the child through the pelvis, and those lying high in the abdomen and not interfering with the pelvis. Tumours occupying the first of these positions are, I believe, always small, and not infrequently solid, or semi-solid in structure, and they may be confounded with fibroid tumours of the uterus and tumours of the Fallopian tubes, or with abscesses and other tumours of the neighbouring parts, or growths from the bones of the pelvis itself. They generally lie in the recto-vaginal pouch of the peritoneum; but Cazeaux quotes a case as having been described by Jackson, in which the tumour lay behind the rectum, pushing it forward. In the absence, however, of all anatomical detail, it is difficult to understand how an ovarian tumour could get into this position. In a case of extra-uterine fetation recently under my observation, the fetus was encysted in the space between the rectum and uterus, and in many respects closely resembled an ovarian tumour. These tumours may be so small as to sink down completely into the cavity of the pelvis, or they may lie in and more or less completely obstruct the brim, and in either case form a very serious complication to labour.

It thus becomes a question of vital importance how labours complicated with tumours of this description are to be dealt with; and it is obvious that the tumour, if large enough to obstruct the passage, must either be shifted out of its place or lessened in size, or the child must be lessened in size, or removed through other than the natural passages.

In a paper of very great value, read by Dr. Playfair before the Obstetrical Society of London, and published in Vol. IX. of their Transactions, the details of fifty-seven cases, published by different authors, in which a tumour of the ovary was pushed down in front of the presenting part, and caused an obstruction to the passage of the child, are collected and tabulated, and the results of the treatment adopted compared. Out of the fifty-seven cases the labour was left to the natural powers in thirteen; and of these, seven of the mothers recovered and six died, being 46·1 per cent.; but in all these cases the tumour must either have been very small or very compressible, and it must have been subjected to an extreme degree of pressure and squeezing in the passing of the child, which may account for the very unfavourable results.

In nine cases the tumour was reduced in size by the evacuation of its contents through a puncture; all the mothers recovered and six of the children were saved.

In five of the cases the tumour was returned above the brim, and all the mothers recovered. In one of these the tumour was apparently solid, and pushed down before the head in such a manner that Cesarean section was contemplated; but Mr. Spencer Wells, who was consulted, succeeded in pushing the tumour up by introducing his whole hand into the vagina, after which delivery was accomplished without further difficulty.

In fifteen cases craniotomy was resorted to, in four of which the tumour had first been punctured; but in two it did not collapse, and in the other two it collapsed sufficiently, but craniotomy was performed because the child was known to be dead. In eight of the fifteen cases the mothers recovered, and in seven they died; but it is expressly noted in several of the cases that recovery was only imperfect. In five cases version was performed, in four of which the mothers died; and in two the forceps was used, and one of the mothers recovered.

It thus appears that no mode of delivery is at all safe in which the tumour is subjected to pressure, whether it be effected by the natural powers, by the forceps, craniotomy, or version; and that for safe delivery, the tumour must either be pushed aside or obliterated by the removal of its contents. The latter of these methods is, no doubt, the simplest and most easily performed, for it is only necessary to puncture it with a trochar through the vagina, and in the recorded cases it has been successful; yet it is not free from objections. In the first place, the tumour is often solid and cannot be reduced in size; and in the second place, we would all be glad to avoid the infliction of even the simplest

wound at the time of labour lest it should give rise to peritonitis or other injurious consequences.

The shifting of the tumour when it can be effected is unquestionably the best practice, though it may be difficult of accomplishment, and sometimes impossible, but still it should be attempted in all cases; and my chief object on the present occasion is to recommend the adoption of a method by which I have myself been very successful in raising pelvic tumours. This method has already been brought before this Society, and consists in the introduction into the rectum of one of Barnes' bags and the slow and forcible distension of it by air or water.

On the 8th of June, 1867, Dr. Ringland read before the Society a case of fibrous tumour of the uterus, in which he and I adopted this method, and succeeded in raising a tumour that had been pressing injuriously on the rectum, and since then I have used it in many cases; one of these was an unmarried lady that Dr. Denham asked me to see along with him, who had a fibrous tumour firmly fixed in the pelvis, and interfering with both the rectum and the bladder, and in which we raised the tumour out of the true into the false pelvis by this means, and effectually relieved the symptoms. In these instances, however, the patients were not pregnant, and the tumours were not ovarian.

On the 27th June, 1869, I attended a lady in labour with her third child, eleven years having elapsed since her previous one had been born. About a year and a-half before I had attended this lady for inflammation of the uterus and ulceration of the os, and during that attendance discovered an ovarian tumour, which lay so deep in the abdomen that she was not herself aware of its existence, though it was larger than the fetal head. When labour commenced I found this tumour occupying the brim of the pelvis, and pushing the os uteri upwards and forwards, so that it was with difficulty that it could be reached. As long as the membranes were unruptured I thought it better not to interfere, as there was no injurious pressure being exercised on the tumour, and I was not without hope that it might be moved out of the way, as in the case of the fibrous tumour so graphically described by Dr. Beatty in his recent volume. However, the membranes ruptured before the os was fully dilated, and instead of moving out of the way, the tumour became more and more tightly pressed into the pelvis. Having asked for assistance, Dr. Churchill saw the case with me. The tumour was now firmly fixed in the brim of the pelvis, it felt hard and solid, and seemed to be quite immoveable, no pressure that we could apply with the fingers making the slightest impression. Puncture of the tumour, and if this failed from its being solid, which seemed very probable, the Cesarian section were almost the only alternatives; but Dr. Churchill agreed with me as to the propriety of trying one of Barnes' bags in the rectum before taking any other step, so we placed the patient on her knees with her head and shoulders

as low as possible, very much in the position described by Thomas for the reduction of a prolapsed funis. The largest of Barnes' bags was now pressed into the rectum and gradually distended with air, while Dr. Churchill kept his finger on the tumour, and, to our very great satisfaction, felt it gradually move as the bag was distended. When it was full I gave it a sudden push, and at once Dr. Churchill said the tumour was gone. It disappeared with a jerk, and the os came fairly into the brim of the pelvis, so we emptied and removed the bag, and made the patient lie on her back till the head was fairly in the pelvis. In a few hours a fine healthy living child was born, and the mother recovered without a bad symptom, and has nursed her child and remained in apparently perfect health ever since.

I conclude this portion of my subject by laying it down as a rule, that in all cases of labour obstructed by a tumour in the pelvis, our first duty is to raise the tumour, if possible, out of the way; and when this cannot be done with the fingers placed in the vagina, it must be attempted through the rectum by some such means as those now described.

Ovarian tumours lying high up in the abdomen are more liable to be injuriously acted on by pregnancy than to interfere with labour, but they may give rise to post partum hemorrhage, or at least make it more difficult to control it when it does occur, as it is not easy to make pressure on the fundus to secure a good contraction as in the following case:—

Ovarian Dropsy and Pregnancy.—Mrs. T., aged twenty-nine, admitted to the Coombe Lying-in Hospital 29th January, 1856. Six years married. Far advanced in third pregnancy, last menstruation having been, she says, in the beginning of April. She now measures, when standing, 4 feet round the abdomen at the umbilicus; when lying on the back, 3 feet $9\frac{1}{2}$ inches.

The abdomen has the appearance of being formed of two spheres, one large occupying the right side, front, and as far as a line drawn from the end of the ninth rib to an inch on the inside of the anterior superior spine of the ilium, on the left side. From this line another swelling extends to the left side, formed by the uterus, and appearing to be a portion of a smaller sphere. The large swelling is soft, even, and fluctuates; the surface is white, and marked by numerous blue veins. The small one regular, firm. It is here she feels the child's movements, and in the upper part of this the fetal heart may be heard, beating about 120 in the minute. The breasts are rather flaccid, but have the areola well developed.

The countenance has a rather worn expression, but is on the whole healthy. General health good, appetite good, bowels regular. The weight of the abdomen is the only thing complained of; to relieve this she wears a bandage.

History.—First child was born ten months after marriage; eleven months afterwards she began to suffer from cramps in the right iliac fossa, which were relieved by aperient medicines. A tumour was soon after found in this region, which, when first observed, was as large as an orange, moveable, and not painful to the touch. She now weaned her child, and in eleven weeks menstruation returned.

She has always suffered much pain in menstruation, chiefly in the right side, "a soreness," which was so great before and during the period that she could not stoop; it was relieved when the discharge ceased. The period occupied a week. For two days before it she suffers from leucorrhea, then the menses appear, with pain for a day or two; then there is an interval of a day without pain or discharge, both, however, returning and lasting a few days. This has been the case before marriage and ever since.

The second child was born two years and ten months after the first. At the commencement of this pregnancy, she says the tumour was the size of a small loaf. After the child was born she was as large as a woman at the full period of pregnancy. She nursed this child two months. Immediately after weaning it the menses returned. When this child was one and a half year old she again became pregnant, and she believes she is now at her full period.

During the interval between the second and third pregnancy she thinks the tumour did not increase in size.

She states that after the birth of the second child, a cyst, larger than the placenta, but not connected with it, containing "corrupted matter," was expelled immediately after the placenta.

24th March.—Labour set in to-day. The child was born as in ordinary circumstances, but after its birth there was considerable hemorrhage, and I was sent for. When I arrived, the placenta had been expelled, and some draining was going on. I administered ergot of rye; and it being impossible to feel the uterus externally, introduced my hand to remove any clots that might have formed, when the hemorrhage ceased. The child was a large healthy well-formed one.

April 17.—Convalescence has been good. She goes home to-day, to return after some time to have the dropsy treated. The abdomen now measures, when lying on her back, 3 feet 9½ inches, and when standing 3 feet 10½ inches. Child thriving well.

In this case we have the history of two pregnancies without any evil consequences from the tumour; and in the case of a lady I was called to see on the 26th May, 1864, the same thing occurred. When I was called in she had been delivered of a living child at full term, and the placenta was expelled naturally; but there was some alarm because

of the size of the abdomen. On examination I found the uterus well contracted, and an ovarian tumour lying above it as large as the uterus at the end of the ninth month of pregnancy. There was no hemorrhage, and she made a good recovery. After delivery the tumour continued to grow, but her general health remained good, and she again became pregnant and went the full period without inconvenience, and was delivered of a living child under my care. She proved pregnant a second time, and was delivered safely, and ultimately died of acute bronchitis.

We have thus, in two patients, the history of five pregnancies without serious inconvenience from the tumours, and so far as they go they show that we cannot accept without hesitation the dogmatic assertion of Dr. Barnes—"That nature would not tolerate the concurrent progress of these two conditions. Nature could hardly bear the simultaneous growth of two tumours like the pregnant uterus and an ovarian tumour. Something must give way." At a meeting of the London Obstetrical Society, in December last, Dr. Braxton Hicks related the particulars of ten cases, comprising eight pregnancies where pregnancy was associated with ovarian tumours, but were all delivered at the full time of living children without danger; but at the same meeting Spencer Wells related fifteen cases that he had seen, but though the results were much less favourable there seem to be still good grounds for hesitating to either induce premature labour, as recommended by Dr. Barnes, or to tap, as recommended by Wells, unless there be special reasons calling for interference.

On Congenital Tumour of the Head in a New-born Infant. By Dr. MacSwiney, Physician to Jervis-street Hospital.

I desire to call the attention of members of the Obstetrical Society, in a very brief communication, to a case which lately came under my notice, where a tumour of the scalp in a new-born infant existed at the time of birth, and presented some features of difficulty and obscurity as to its exact diagnosis. The history of the case was, shortly, as follows:—

Mrs. L. was confined of her eighth child on December 9, 1869. Her labour was natural and easy, it was speedily accomplished, and did not require instrumental aid. The infant was a large, well-nourished male, apparently very healthy. Nothing particular was noticed in the appearance of any part of the child's body of an abnormal character on the day of its birth. However, on visiting this lady the next day after the birth of the child, my attention was called by the nurse to something wrong about the infant's head. Upon examination I found there was a tumour situated upon the right side of the cranium, over the right parietal and part of the occipital bones, near their upper, or vertical, margin. This was covered by the scalp, which was of its natural colour, and well supplied with hair. It was round in outline, broader at the base than at

the apex, and had a dimension of $5\frac{1}{2} \times 4\frac{1}{2}$ inches. Its size was not increased when the child cried. It was full, firm, fluctuating, and elastic. Gently manipulating it, for the purpose of examination into its nature, did not appear to give any pain, and slight pressure did not in the least diminish its size. I made but slight pressure, as I was unwilling to ascertain experimentally, by any forcible compression, whether or not the contents of the tumour communicated with the cavity of the cranium. The cranial bone could not be felt at the bottom, but then, indeed, the tumour was so full that no degree of pressure which was justifiable with the fingers on its surface, could enable the examiner to reach to a level with the bones. Its base was clearly circumscribed by a thick ring of bony hardness, and the finger carried over this well marked edge or ridge, dipped into what appeared to be a bottomless cavity. I repeatedly tried to satisfy myself on the point, whether or not a communication existed between the external swelling and the meninges, with the result that I always thought nothing could be more complete than the resemblance presented upon each such examination to an absence of bone under the tumour, and, on the whole, I altogether inclined to the opinion that such osseous deficiency did really exist, notwithstanding that there was no pulsation in the tumour, nor were there any symptoms of compression present, facts in the case which, it is needless to say, told strongly against this opinion. When the infant was three weeks old, I asked my friend, Dr. T. M. Madden, to examine the tumour with me, and the following is a short extract from a note which he has written me :—"The edges were sharp, and it felt as though there was an actual deficiency of bone in the centre, and such was my opinion at the moment." I adopted the expectant plan with regard to this tumour, and no treatment whatsoever was had recourse to. For five weeks the tumour remained stationary, at least in so far as that not the slightest diminution in its size took place; indeed, in the opinion of the mother and nurse, it became larger, and I am inclined to think that this may have been the case. However this might be, at the expiration of these five weeks a process of absorption was suddenly set up, without any known cause, and in two days the tumour entirely disappeared, when the cranial bones were found perfect. The child has remained since then in complete good health, except that it suffers from an attack of moist eczema of the scalp.

I was at first at a loss to determine with what class of tumours to identify this swelling. I was familiar with the ordinary bloody tumour of the scalp, between the pericranium and tendon of the occipito-frontalis, sometimes met with in new-born children, and I, for a while, thought of referring it to this class. But the scalp tumour usually occurs in cases where the labour, mostly in a primipara, has been prolonged, or where instrumental delivery had been effected. No such exciting cause, however, existed in the present instance. The tumour, also, was more tense

and elastic than the scalp swelling usually is. Moreover, in ordinary *cephalematoma* the active vitality of the integuments of the head soon cause the absorption of the effused blood, whilst the lapse of many days found this tumour unaltered in size or appearance. I therefore, decided against its being the common scalp swelling. I next asked myself was it a *meningocele*, or was it an instance of that rare malformation, an *encephalocele*. Ordinarily the diagnosis of these congenital abnormalities is not difficult. Their situation generally in the middle line and at the occipital region, their pedunculated form, the symptoms their presence induces, the state of the infant's general health, the effect upon them of the crying or coughing of the infant—all these afford means sufficient, in most instances, to enable the physician to discriminate between these congenital protrusions of the cerebral contents, and other tumours on the scalps of new-born children. But then other situations are sometimes occupied by the "hernia cerebri congenita" (*Niemeyer*); in form it is often sessile, and cases, therefore, occasionally occur in which the diagnosis is not so easy. Thus, M. Giralde's (*Comptes rendus de la Societié de Biologie*), relates a case where a congenital dermoid cyst, or tumour of the scalp, was mistaken for a meningocele. Fluid was evacuated by an operation from this cyst, with a view to determine its nature, and was found to be identical in appearance with cerebro-spinal fluid. Its real nature, however, namely, that of a dermoid cyst, was subsequently made quite manifest. Again, I was at one time inclined to think that the case I am relating might have been a meningocele, whose communication with the skull had become obliterated. A preparation exists in the Museum of the St. George's Hospital, London, in which a spontaneous cure of a meningocele occurred.—(*Holmes*, p. 68). I also considered the question whether the fluid contents of this swelling was purulent, as it possibly might have been, and I concluded that it did not contain pus, as there had been no inflammation of the soft parts, and the feeling of fluctuation was unlike that communicated by an abscess.

Finally, I arrived at the opinion that this was an instance of a class of tumours whose occasional occurrence has been demonstrated, that it was, in fact, an example of an intra-uterine extravasation of blood between the pericranium and bone, the result of injury. There was, in fact, a circumstance, now to be related, in the history of the mother before the birth of the child, which much influenced me in coming to this conclusion, and which I submit to the judgment of the Society. Respecting the etiology of this class of tumours in general, I do not presume to advance any explanation, but with regard to the cause of the production of the swelling, in the particular case now under our consideration, I am able to present something definite by mentioning an accident which occurred to the lady about a month before parturition, which, I have no doubt, played an important part in causing the tumour to form. Mrs. L., wishing to adjust a window curtain, stood on a rather high chair. Whilst thus

engaged, one of the legs of the chair suddenly broke, and she fell to the floor, striking the lower part of the front of the abdomen heavily upon the edge of the back of the chair. She, of course, received a great shock and felt much hurt and frightened. She fully expected that premature labour would supervene. She lay upon a sofa for some time, and as nothing serious occurred, and the pain gradually subsided, she became reassured, and, after the lapse of a few days, thought no more about it, until asked by me if she knew any way of accounting for the swelling on the child's head, when she related to me the particulars of the accident which I have just brought under the notice of the Society. It is not for me to anticipate whether members will agree with me in ascribing the production of the tumour to this fall.—*Read 12th March*, 1870.

On a Remarkable Case of Complex Labour. By Thomas More Madden, M.R.I.A., L.K. & Q.C.P.I., M.R.C.S. Eng., Assistant-Physician to the Dublin Lying-in Hospital (Rotunda).

The following case being unparalleled, as far as I can ascertain, in the annals of midwifery, will probably be considered of interest by this Society.

February 9th, 1870.—I was sent for, at 7. 30 p.m., to visit J. S., of 7, Moore-street, a patient attended by the pupils from the extern maternity of this hospital. She had been in labour, and the liquor amnii had been evacuated for twenty-four hours. The pupil in charge of the case (Mr. E. B. Roche), to whose notes I am largely indebted, was not sent for till an hour previously, *i.e.*, 6·30 p.m., when on examining her he found about three inches of the cord prolapsed, and two feet, with a rounded tumour behind, then presenting. He, not unnaturally, concluded this was a breech; and as the cord was pulsating strongly endeavoured to return the prolapsed funis. In doing this he discovered, to his great surprise, that the rounded tumour he felt was not the breech but the head tightly jammed down through the brim by the closely contracted uterus. He then made a decided attempt to push the feet and cord above the head, but so tightly were they caught between the head and the pelvis that he could make no impression on them. He now sent for assistance; and endeavoured in the meantime to keep the pressure off the funis. On my arrival I found Mr. Roche's account of the presentation perfectly accurate. "The feet were presenting at the posterior part of the vagina, the heels to the sacrum, the toes pointing directly forwards. The feet could be circumscribed and followed up to the lower part of the legs, where they were tightly caught between the head and the promontory of the sacrum. There was a loop of the funis, about three inches, prolapsed between the legs, and pulsating strongly in the intervals of the pains. The head lay in the second position, or rather more transverse, and was jammed down upon the feet and cord;" the pulsations in the latter now ceasing during each pain. The uterus being very tightly contracted on the fetus, and

the liquor amnii having been evacuated for 24 hours, and knowing from experience on other occasions the great difficulty of performing version under these circumstances, I proceeded to apply the long forceps, in the hope of bringing down the head past the feet; they however failed to make any impression, although I tried them in the occipito frontal as well as in the transverse diameters, and as they slipped in both, after several attempts, I abandoned them.

The funis was still pulsating, but less strongly than when I first arrived. She was now put under the influence of chloroform, by Mr. A. C. Roberts, and this had the effect of so far relaxing the parts that I was enabled, though with considerable difficulty, to draw down one foot; this I could not do before the chloroform was given. The foot was then secured by the attending pupil whilst I reintroduced my hand, and, using some force, succeeded in pushing back the head through the brim of the pelvis and into the uterus, at the same time making forcible traction on the foot. After these efforts had been continued for some time without apparent effect, the fetus suddenly rotated, the foot came down, the head receded, and the delivery was now easily accomplished. The child was born alive, though apparently still-born, and was resuscitated by the diligent application of the ordinary measures. There was a deep indentation on the right parietal bone where the head had been compressed between the feet and the pelvis. She made a good recovery, and some days after I learned that mother and child were doing well.

There appear to me to be two points of interest in the foregoing case; the first is the extreme rarity of a compound presentation such as this; the second is the length of time which had elapsed between the rupture of the membranes and the birth of a living child in this case of prolapsed funis.

The extreme rarity of a case such as that just described may be inferred from the fact that in none of the statistical reports of the Rotunda Dublin Lying-in Hospital, which, conjointly, afford the largest, and perhaps the most accurate, mass of obstetric statistics in existence, is there any mention of a similar case of complex labour. Thus Dr. Collins, who gives an account of 16,654 deliveries, does not allude to such a complication. Neither do Drs. Hardy and M'Clintock in their report of 6,634 labour cases; nor is there any similar case in Drs. Johnston and Sinclair's account of 13,748 deliveries. Thus in 37,036 cases which are reported on by the Masters of the Dublin Lying-in Hospital, no case such as that now laid before the Obstetrical Society occurred. Dr. Churchill in the last edition of his *Theory and Practice of Midwifery*, although he devotes a section to the consideration of "Compound Presentations," does not refer to any case similar to that which form the subject of this paper. Nor is there such a case described in Dr. Ramsbotham's *Obstetric Medicine and Surgery*.

Mauriceau is the only writer that I know of who records a case nearly analogous, and his observations on this, as well as other cases of compound presentations which he describes, although made some two hundred years ago, are so judicious, and so well worthy of being borne in mind, that I shall make no apology for translating two of his cases which appear to me to bear on that which has just been laid before the Society :—

"Observation XLV.—De l'accouchement d'eme femme dont l'enfant presentoit le coste de la teste avec un pied et chute du cordon de l'ombilic.

"On the 29th of October, 1671, I delivered a woman of a large child, which presented the side of the head with a foot and prolapse of the funis as far as the external orifice of the vagina, in which cord evident pulsation was to be felt, which made me know that the child was still alive, although it had been nearly two hours in this unfortunate situation before the midwife who assisted the woman sent for me to aid her. But what is worthy of note in this case is that it was the foot of the child that presented with the head, which saved this infant from perishing, by preserving the umbilical cord from entire compression by the round head of the child, as must have occurred if the cord had presented by itself together with the head, which compression, by interrupting the circulation of the blood, that should there take place freely, must of necessity have caused the death of the child, which I, however, delivered alive by drawing down the feet, after having a little pushed back the head with the umbilical cord, in order to make the extraction more easily." [a]

"Observation CXLV.—On the 17th of October, 1675, I delivered a woman of a very large living child which had the feet and hands strongly engaged in the passage with the head when I was sent for to her assistance. Her midwife had previously endeavoured to draw down the child by a foot, but instead of doing so had but more firmly empacted the head in the passage for want of having returned it within the pelvis, as well as the hands which also presented. Before drawing down by the feet, as I did in her presence, making her understand that in these operations the good judgment of he who works is still more necessary than strength of body, which she said she had not, in order to be able to affect those laborious deliveries." [a]

With regard to the treatment adopted in the foregoing case I shall make a very brief allusion. I have been told that considering the length of time the liquor amnii had been evacuated, and the tightness with which the uterus was contracted on the fetal body, that I was not

[a] Observations sur la Grossesse et l'Accouchement des Femmes, et sur leurs Maladies. Par François Mauriceau, &c., p. 29. Paris, 1695.

justified, having a due regard to the safety of the mother, in returning the head into the uterine cavity, and performing version; and a strong hint was thrown out that I should have used the perforator under such circumstances. Other eminent obstetricians, whose opinions on any question connected with midwifery are entitled to great weight, found some fault with my practice in having first attempted to apply the forceps in this case, and not having at once brought down the feet, and I much regret being obliged to differ from their views, as I must do on this question, for though "*Amicus Plato, amicus Socrates, sed magis amica veritas.*" It is a great satisfaction to me, however, that my practice meets with the full approval which has been expressed by the accoucheur who is officially the head of our department of the profession in this country, the President of this Society, and Master of the Lying-in Hospital. I still think that what I did was right, and had I such a presentation to deal with again I should again act as I did in the foregoing instance, and should be well satisfied if I again saved the lives of both mother and child under such circumstances. I shall not stop to discuss the hint thrown out as to the necessity of perforation. My views on this point are too clear to admit of a shadow of a doubt upon it. I do not think it morally right to destroy any living human being. Most assuredly then I do not regret not having destroyed a child which, with safety to its mother, I was enabled to deliver alive.

With regard to the opinion that I should not have attempted the forceps in this case, I would only remind the Society of the fact that the head of the child having passed through the brim was lying in the pelvic cavity, within easy reach of the forceps, and in the second position; that the feet were far up at the brim of the pelvis and that the waters had been evacuated twenty-four hours before I was sent for. It is obvious that the forceps if successful would, under these circumstances, be applied with greater safety to the mother than generally attends the operation of turning. As to the rule laid down by some writers of never applying the forceps in funis presentations lest the cord should be compressed thereby, I must say that I attach no practical importance to it. No person should ever attempt to apply the forceps who does not possess sufficient dexterity in their use to avoid the possibility of so untoward an accident. The question of turning or applying the forceps in funis presentations must be decided in every instance by the circumstances of each particular case, and if the head be low down in the pelvic cavity I think the forceps should be tried before resorting to version. That the funis was not compressed by my application of the forceps is proved by the fact that the child was born alive after their use.

I may also mention, that in the same week in which I attended the patient of whose labour I have just given the history, no less than four cases of funis presentation came under my observation; in one of these I

delivered by the long forceps, in the others I performed version, and in all of them the child was born alive. I need not remind this Society that in cases of funis presentation the statistics of our hospital and other similar institutions show that more than one-half of the total number of children delivered are born dead in consequence of pressure on the cord during labour. Therefore, if the foregoing case occurred again I should again attempt to use the forceps before turning; but, before doing so, I should follow the recommendation of Dr. Johnston and Dr. Denham, and put my patient under the influence of chloroform, by which, perhaps, the parts might be sufficiently relaxed to enable the extraction with the forceps. This failing, version must be resorted to; and the operation in question certainly amounted to something more than version. It was not a mere extraction of the child by drawing down the feet, for the head had to be moved out of the cavity of the pelvis and passed back through the brim into the uterus, whilst powerful traction being exerted by myself and my assistant on the feet, the body of the child was made to rotate. The difficulty of doing this in a case where the uterus was so long, and so powerfully contracted around the child's body, must be obvious to any obstetrician; and none but one who had his hand in a uterus firmly contracted on the child can well judge of the amount of force which may be employed, and suffice to overcome the strength of the contraction, without running the danger of thereby rupturing the uterus. It was very satisfactory to find that in this case the mother did as well as the child, and convalesced rapidly.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

Dr. M'Clintock, President.

Aortic Aneurism.—Dr. William Moore said, the morbid specimen he now exibited was taken from the body of a man, 31 years of age, who had been admitted to Sir Patrick Dun's Hospital on the 31st December. About three years previously he was engaged in a very laborious occupation. He was employed in coaling the Cunard steam ships in Liverpool, and had often worked continuously for 24 hours, during which time 120 or 130 tons of coal would be put into the ship. On one occasion he fell into the hold of one of these ships, about 20 feet. A short time afterwards he for the first time complained of pain in the back; on resuming work he found that he had this incessant pain in the back. He came over to Dublin and placed himself under the care of Mr. Tufnell, and at that time he complained also of palpitation in the epigastrium. Under treatment he got rid of this palpitation, and resumed his work, but he was

obliged to give it up and came back to Dublin, where he was again under the care of Mr. Tufnell for eight months. He then returned to Liverpool. When he came under Dr. Moore's care he was very pale, and complained much of pain in his back. He suffered from pain in the region between the ensiform cartilage and the umbilicus extending up under the left floating ribs. He passed his urine freely and naturally; it was of normal specific gravity, and contained no albumen. There was no jaundice, nor any œdema of the legs or feet, or ascites. The pulsation was feeble in both iliac arteries; there was visible pulsation in the radial arteries. The pulse was 84. On making a more careful examination, dulness was found over the pericardial region, and a peculiar click, an intensified throb, so to speak, but no bruit. There was visible pulsation below the ensiform cartilage, and dulness on percussion. On examining the same region between the ensiform cartilage and the umbilicus with the stethoscope when he was recumbent, a distinct bruit was audible. If placed in an upright position the bruit became inaudible. On being turned on his face, at the level of the last dorsal vertebræ a well marked bruit could be heard, particularly at the left of the spine, which became intensified, until the fourth lumbar vertebra was reached. When placed on his hands and knees he did not lose the pulsation in front. Matters went on thus for some time, and the man became quite cheerful, and every day he got up and went about the wards. On the day he died, he was going about as usual, when he called out suddenly, "Send for the doctor," but before the resident pupil could come to him he was dead.

The *post mortem* examination revealed general hypertrophy of the heart. There was frothy mucus in the larger tubes, and a rather congested condition of the lungs. Upon examining the abdominal aorta, an aneurism was found which was as large as an orange. It was given off immediately at the celiac axis. It had obliterated the renal artery, and the right kidney was reduced to an ounce in weight.

The case was interesting in many points of view. Had they contented themselves with examining the patient over the front of the abdomen they would have perceived nothing which could not be accounted for by the existence of a tumour, a psoas, or hepatic abscess; the indications to be observed in front were common to those tumours that pressed on the vessels; but if they placed the man on his hands and knees, the tumour would fall forward, and the bruit would disappear. In the majority of such cases there would be a total absence of bruit when the patient was placed on his hands and knees. In this case, owing to the persistence of the bruit, in the prone position, coupled with the other symptoms, they had no difficulty in diagnosing it as a case of aneurism. He might be asked was this persistence of bruit pathognomonic of aneurism? It was not; but its being confined to two or three inches along the spine from the

last dorsal to the fourth lumbar vertebræ, prevented it being confounded with spanemic or obstructive aortic murmurs, which would have been continuous from the nape of the neck downwards.—*February* 6, 1869.

Cerebro-spinal Meningitis.—Dr. Grimshaw exhibited the brain and spinal cord of a female, aged twenty-one, who had died in Steevens' Hospital, under the care of Dr. Swan. She had been a fortnight ill before she was admitted to hospital, on the 30th of January. She had some nausea, headache, and general uneasiness, together with some slight cerebral symptoms. The only peculiarity about her was that she got up once or twice during the night and walked about, in consequence of great pain in the head. These attacks were sudden. Some days before admission to hospital she got diarrhea and vomiting, the diarrhea being of such a nature as to make one suspect that she had typhoid fever. On admission to hospital she was vomiting, and had still diarrhea. The pain in the head of which she complained was very considerable; she had pains in the shoulders also, and in the back of the neck. In two days after admission, on the 2nd of February, she had strabismus of both eyes; the pulse was 96, the temperature 101·2. During this day she became quite comatose, and towards evening muttering delirium. Her pulse was then 120. He (Dr. Grimshaw) saw her on the following morning. He found her with a quick pulse of a peculiar jerking character; the head slightly drawn back; no stiffness of the limbs; quite insensible; the vomiting of the peculiar green colour seen in these cases. He came to the conclusion that the case was one of meningitis, although the symptoms were not well marked. The vomiting had ceased, but the diarrhea still continued. On the following day she was worse, and manifested another symptom of meningitis; hyperesthesia well marked on the right hip, and along the right thigh. On the 5th she was still worse, and died on the next evening. The temperature before death was 99·6; the pulse was imperceptible. She had not passed water during the two days preceding her death, except with the assistance of a catheter. No spots of any kind appeared on the body. The case was remarkable because of the slowness of the invasion of the disease.

The *post mortem* examination was made that day (6th February). The rigor mortis was well marked; the whole of the body was covered with dark purple patches, none of which, as he had already said, appeared before death. On making an examination of the brain and spinal cord, a large quantity of the usual dark fluid blood exuded from the vessels. The cord presented a considerable amount of inflammation. There were many patches of lymphy effusion. The cord was rather firmer than usual; the upper part of the cord had not the pathological characters so well marked as usual in these cases; they were more marked in the brain than in the cord. The arachnoid over the base of the brain was opaque,

and at one point a mass of greenish yellow deposit was present. The arachnoid at the upper surface of the cerebellum was opaque, and there was a considerable amount of deposit; the whole of the sub-arachnoid space was filled with fluid; there was a great deal of adhesion between the hemispheres. The intestines presented no pathological change indicative of typhoid fever.—*February* 6, 1869.

Ovarian Tumour.—Dr. Bennett exhibited an ovarian cyst which he had removed from a body in the dissecting-room of Trinity College. He had no knowledge of the history of the case during life, and exhibited the tumour to the Society on account of the remarkable chemical peculiarity of its contents, and some points of anatomical interest in its structure.

The tumour was very prominent in the abdomen of the subject, and projected much to the right side, and two or three irregular projections could be felt to the left side, and in the pelvic region. On opening the abdomen the tumour was found to be entirely adherent in front to the abdominal wall, while it was free from connexion with any of the abdominal viscera. On detaching it from the abdominal wall it was easily raised up, and was found to have a very long pedicle formed of the broad ligament of the uterus. The pedicle was remarkably twisted, apparently by a complete rotation of the tumour during its formation. This twisting had nearly divided the Fallopian tube, and must have caused much compression, for a time at least, on the vessels of the pedicle. The Fallopian tube, being attached to the upper part of the tumour, was greatly elongated, being fourteen inches long. It was attached to the tumour by a distinct process of the broad ligament in its entire length, and was pervious throughout.

The tumour, with its pedicle, when removed from the body, weighed twenty-five pounds. It was a multilocular cyst of the second variety described by Hodgkin. A great quantity of fluid was drawn off from the principal cyst, which was divided in many places by septa, the remains of cysts which had apparently burst into the principal cavity. The fluid was dark, of the colour of coffee grounds, and had a distinct bilious odour. The smaller cysts contained a dark fluid, not as turbid as that of the large cavity, but in all possessing the same odour as it. On allowing the fluid to rest, there were seen abundance of crystals of cholesterine. The odour of the fluid was so striking that Dr. Bennett submitted it to chemical examination in the laboratory, with a view to determine the presence of the biliary acids. The fluid was treated so as to coagulate the albuminous matter, and filtered. The clear fluid so obtained, submitted to Pettenkofer's test, gave the most unequivocal reaction, showing the presence of the biliary acids. The experiment was repeated on the contents of several of the smaller cysts, under Dr. Apjohn's personal direction, with the same result.

The fluid had, in addition to the chemical reaction and odour, the bitter taste of bile. Dr. Bennett considered the case worthy of being brought before the society, as he was not aware that any case had been recorded of the presence of the biliary acids in the fluid of ovarian cysts. There was no trace of connexion in this case between the tumour and the biliary passages, or liver or intestines, nor was there any trace of jaundice present. The twisting of the pedicle, and the great length of the Fallopian tube, were also worthy of notice.—*February* 13, 1869.

Gall-stone.—Dr. MacSwiney said the case he had to bring under their notice illustrated one of the dangerous effects, ending with death, which the action of gall-stones in the human gall bladder was capable of producing. The records of that Society and the pathological museums of that and other cities exhibited abundant evidence and examples of various, serious, and sometimes fatal results, caused by gall-stones. In this way—pyemic abscesses, inflammation of the biliary passages, peritonitis, fistulæ and other grave, and often mortal lesions had been produced. The case he now presented was an example of a fatal result from the action of a gall-stone in its transit from the gall bladder. The patient was aged 50. She entered Jervis-street Hospital on that day week. She stated that she had been in the enjoyment of remarkably good health up to the period when her last illness commenced. The only drawback to her general good health was that she suffered occasionally from what she called windy colic, which submitted to homely treatment, such as warm applications to the abdomen and a warm drink. On Thursday week last she said she was perfectly well in the morning, eat her breakfast heartily, and very shortly after it she was attacked with a violent pain, which she referred to the right side under the ribs. This pain was succeeded very soon by severe vomiting, and she stated she continued in a condition of great suffering, vomiting everything which she took during the whole of Thursday, the whole of Friday, and up to the afternoon of Saturday, having consulted no doctor, and relying on ordinary household remedies, a mustard plaster, &c. Dr. MacSwiney saw her on Saturday evening after her admission. Her condition was as follows:—She was slightly jaundiced. Her face, hands, and feet were perfectly livid, and quite cold, and she was bathed with a cold sweat. There was no pulse to be detected at the wrist, and when he had to determine the circulation he had to apply the stethoscope over the region of the heart. Her tongue was coated with a yellowish white fur. Her thirst was intense. She vomited everything that was given to her, cold water, iced water, brandy and water, chicken broth. The state of the abdomen was this. It was tumid in a remarkable degree. It admitted, speaking generally, of palpation, or compression with the fingers, without any inconvenience or complaint. It was tympanitic in a remarkable degree and a space about two or three square inches at the right side

immediately below the ribs, and extending into the epigastric region was hard, dull on percussion, intensely painful, and this pain was greatly aggravated by movement. In fact the character of her pain was remarkable. It was of a threefold nature, like that which is so generally regarded as the pain that characterizes biliary colic, it consisted of a persistent constant uneasiness, added to which were paroxysmal attacks of intense suffering, intermitted by periods of repose, and in addition to this there was the fact that the slightest movement aggravated her sufferings exceedingly It was quite plain that there was a local circumscribed peritonitis, and it was assumed that there was some obstruction to the peristaltic action, whether of a paralytic or of a mechanical nature he could not say. He suggested that she might be passing gall stones. She continued to gradually get worse, and died on Monday morning, 36 hours after entering the hospital, retaining her intellectual faculties to the last.

A *post mortem* examination was made. She was without exception the fattest corpse he ever saw opened, and that had a direct bearing on the nature of her malady. Upon opening the abdomen the signs of recent peritonitis in a circumscribed area were very easily discerned. The upper surface of the liver was attached by recent lymph to the surrounding parts; the great omentum which projected outward was attached very slightly to the parts with which it was in immediate contact. The lymph was remarkable. It was green, the colour of bile; it was obvious there was a considerable quantity of bile poured out which had been mixed with the lymph. The adhesions were easily broken up and were evidently quite recent. In the stomach there was a quantity of greenish, mucous looking fluid. There was no solid matter of any kind that he could determine. It was found that the stomach was adherent by very recent adhesions to the gall bladder, at the place where the gall ducts unite to form the common duct. Upon detaching these adhesions it was found that there was a large pouch at the junction of these ducts which was full of bile; the gall-bladder contained a number of polyhedral gall-stones without a drop of bile. There was corresponding to the adhesion to the gall duct an opening into the duodenum which admitted of the handle of a knife. Tracing down the common duct to its opening in the duodenum it was found to be considerably enlarged in calibre, and at its extremity just where it opened into the duodenum there was found firmly imbedded a large gall-stone. This could be recognized on the internal surface of the intestine and felt with a probe, but he was unable to pass the probe from the intestine into the gall-duct, and was unable to say whether this passage was pervious or not. The condition of the parts was this:—The gall-bladder was full of gall-stones and empty of bile; there was an adhesion between the duodenum and the lower end of the gall-bladder; there was perforation there; there was an

effusion of bile which was recognized in the recently effused lymph, by its green colour, and finally there was a large gall-stone embedded at the end of the common duct. The sequence of pathological events was doubtful, but he supposed that the large gall-stone which was found at the end of the common duct must have been the initial object in the changes that had taken place. Its position might have been taken up by it for a considerable period antecedently to death, and the bile may have trickled in small quantities through it into the duodenum, or it may have completely closed the duct, producing an accumulation of bile in the gall-bladder, irritation of the biliary passages, adhesion of the duct to the duodenum, and effusion of bile through the ulcerated opening.—*Feb.* 13, 1869.

Intra-thoracic Malignant Tumour simulating Aneurism.—Dr. Little said that the interest of the specimen he now exhibited depended altogether on its clinical history. In the early part of October a man came under his care in the Adelaide Hospital, aged fifty-four. He was a fine specimen of the English mechanic, and had been employed in India, Egypt, and elsewhere abroad. He had been three years in Dublin. His health had been good until about a year before he sought admission into the hospital. He then spat some blood. After this he lost flesh, and felt unable for much exertion, but had no marked symptoms of illness. Six months before admission to hospital he began to suffer from difficulty of swallowing. When he eat, the morsel, he said, stuck behind the breast bone low down. After this had continued for some time another symptom arose. He lost his voice, and became subject to a constant tickling cough. On admission to hospital he was somewhat emaciated and weak. The marked symptoms were slight difficulty of swallowing, aphonia, constant tickling cough, which brought up mucus tinged with blood, wandering pains about the chest, but particularly a pain on the left side, between the spine and the scapula. On a physical examination of the chest there was but one departure from health discovered, diminution of the respiratory murmur in the left lung. He ventured to make the diagnosis of an aneurism of the aorta involving the left angle and descending portion of the arch. In a fortnight the man's friends took him out of the hospital. He (Dr. Little) kept him under observation, and went to see him now and then. He gradually became worse, worn out by the incessant cough, which prevented him taking food or sleeping, and died in the beginning of January. Soon, however, in the progress of the case, there were signs which made him doubt the accuracy of his original diagnosis. There came dulness on percussion in the front of the chest, and that dulness spread to the left of the sternum, not to the right. Now they knew when an aneurismal tumour was making its way to the front of the chest it generally went to the right of the sternum and not to the left.

There was no impulse, no arterial sound, over the dull region. There was another circumstance which made him doubt that it was an aneurism. The man became steadily worse. Now, in an aneurism, they knew that the course from the commencement of the disease to the death of the patient was intermittent: the patient would get better, then worse, then improve, then get bad again. But there was a third circumstance which made him doubt that the man had an aneurism, and he mentioned it because of its diagnostic significance—the application of leeches to the chest gave no relief. They knew that no matter on what structure the pressure of an aneurism was exerted, whether on the œsophagus, a bronchial tube, or the wall of the chest, the application of leeches gave at least temporary relief.

On opening the chest he found the man had no aneurism, but a large encephaloid growth, a portion of which he now exhibited, and which occupied the whole of the anterior and posterior mediastinum, and which he believed had its origin in the posterior mediastinum. At a late meeting Professor Moore showed a specimen of aneurism of the aorta, the existence of which had not been indicated during life by any of the ordinary symptoms; and, as a contrast, this case was interesting as showing that they might have all the symptoms that pointed to aneurism—difficulty of swallowing, aphonia, lessened respiratory murmur in one lung, constant cough, bringing up mucus tinged with blood, and yet that the disease present might not be aneurism. The fact that the leeching of the chest did not give any relief was the first thing that made him suspect there was no aneurism. Since, several years ago, he had heard Dr. Stokes tell of a patient with thoracic aneurism, who came for many months to the Meath Hospital to have leeches applied on account of the invariable relief they afforded, he (Dr. Little) had been greatly struck with the constancy with which the withdrawing of a few drachms of blood, in cases of aneurism, gave at least transient relief to the pressure symptoms, whether dysphagia, dyspnea, or pain, an effect so constant that its non-occurrence in the case he now detailed made him doubt the diagnosis, notwithstanding how strikingly suggestive of aneurism were the symptoms.—*February* 13, 1869.

Disease of the Mitral Valves.—Dr. Hayden submitted to the Society two hearts illustrating the subject of constriction of the mitral orifice, which, in Dublin at least, was a comparatively new subject, and one upon which there was still a great difference of opinion. These specimens were somewhat old, but he thought the subject was new and interesting. The first of these hearts was taken from a boy ten years of age, who was admitted into the Mater Misericordiæ Hospital under his care on the 3rd of February, 1868. He learned that five years

previously the boy had scarlatina, and spat blood. When he saw him for the first time the pulse was scarcely perceptible. He had quick respiration, cough and mucous expectoration. He spat blood about once every three weeks, the hemoptysis continuing three days, and always attended with considerable alleviation of his symptoms. He remained under treatment a short time, and left the hospital at his own request, much relieved, but without any alteration in the physical signs. The prominent physical sign was a pre-systolic murmur at the apex of the heart, and from this he had no hesitation in diagnosing constriction of the mitral orifice. The patient presented himself again at the hospital in the end of February, and also on the 23rd of March. There was no change, except that on the last occasion he (Dr. Hayden) discovered at the apex of the heart both a systolic and a pre-systolic murmur. He continued to spit blood in large quantities, and was re-admitted on the 22nd of June. He was then unconscious, his face puffed and pallid, lips livid, no pulse at the wrist, the pre-systolic murmur less prominent, but still distinct. He died in a few days, and, on a *post mortem* examination, the following was found to be the condition of the heart :— There was very little alteration in the right cavities. The right ventricle was a little dilated, and the auriculo-ventricular orifice considerably so, three fingers passing through it easily. The left auricle was considerably dilated and thickened. The mitral orifice was much reduced in size, so much so that it barely admitted the barrel of a quill. The left ventricle was in a normal state, neither dilated nor thickened. The pericardium was found firmly adherent to the heart.

The second case was that of a female, aged thirty, who came up from the west of Ireland to be treated for jaundice. She complained of no other illness whatever. The jaundice appeared for the first time four months previously, and one month after her first confinement. There was no complaint of pain, nor anything to attract special attention to the chest. The pulse was 84, the skin of a deep yellow tint. She passed urine in large quantities. There was scarcely any tenderness in the hypochondriac region. In a hurried examination of the heart he thought he detected a systolic murmur. The symptoms, however, were not of a kind to attract any further attention to the heart, and he did not repeat his examination till the 1st of July. She had then undergone considerable improvement. The jaundice was less decided. She mentioned, however, that she had a " fluttering about the heart," and he made a careful examination of the organ in the corridor of the hospital, where she was taking exercise at the time, and at once declared the case to be one of constriction of the mitral orifice, in the absence of all symptoms and signs of organic disease of the heart, save the pre-systolic murmur.

On the 3rd of August she was struck down by typhus fever. He saw

her on that morning; the pulse was then 120. The murmur at the apex of the heart was still more decided in character. On his return from Oxford on the 11th August he found her body covered with dark spots, interspersed with petechiæ. She died on the following day. There was about a pint of fluid found in the pericardium. The heart weighed eleven ounces and a half. The right cavities were in the condition already mentioned when speaking of the first case. The right auriculo-ventricular orifice was dilated as in that case, but the right ventricle had undergone no change either as to the thickness of its walls or the size of the cavity. The left auricle, as was invariably the case, was enlarged and thickened. The mitral orifice was constricted, admitting easily the point of the index finger, and converted into a button-hole slit by partial agglutination of the valves. The chordæ tendineæ were shortened and thickened. The left ventricle had undergone no change, being of the ordinary size and thickness.

He should not further dwell on these cases, except to say that they afforded a favourable illustration not only of the practicability of diagnosing constriction of the mitral orifice, but likewise of its comparative facility. In the latter of the two cases there was no evidence whatever of disease of the heart, with the exception of a single sign, yet from this single sign he had no hesitation in pronouncing, with perfect confidence, the diagnosis of mitral constriction.

One or two features were worthy of notice. First, the suppression of the pulse in the greater number of these cases, and the remarkable apathy in regard to suffering: the patient's walking about the ward, with no pulse perceptible at the wrist, and declaring, in answer to inquiries concerning their health, that they were better; and lastly, the frequent recurrence of hemoptysis, which was a symptom to be met with in every one of these cases.—*February* 20, 1869.

Cancer of the Liver.—Dr. Cryan said that James W—, a wool-packer, aged 52, was admitted into St. Vincent's Hospital, under his care, on the 19th of December, 1868. For the past 6 or 8 years the patient has been in the habit of drinking from two to three quarts of porter daily, but he, nevertheless, usually enjoyed good health up to seven months before the period of his admission into hospital; he then began to complain of want of appetite, occasional nausea, and gradual loss of flesh and strength. On admission he looked pale and anemic, and was much depressed in spirits; he was greatly emaciated, and complained of loss of appetite, nausea, flatulence, and extreme debility; there was slight tenderness over the epigastric region, but no pain there, or over the liver; he had never vomited blood, or passed any from the bowels, nor had he been affected with syphilis, or disease of the bones; the liver could be felt projecting fully an inch and a half below the right costal arch; it was

hard and smooth, and slightly tender on palpation; the hepatic dulness, in the right mammary line, measured $6\frac{1}{2}$ inches; there was no evidence of splenic enlargement; the peritoneum contained a small amount of fluid; the girth of the abdomen, at the umbilicus, measured 34 inches; the superficial veins of the abdomen were not enlarged. The urine was scanty—about 20 ounces—sp. grav. 1032, acid, dark like porter, and threw down a copious deposit of lithates; it contained some bile pigment, but no bile-acids, and no albumen; tongue moist and covered with a thin white fur; bowels free; motions contained plenty of bile, and were not offensive. The respiratory signs were normal.

The impulse of the heart was very weak, the *first* sound short and clear, resembling the second in these characters; the area of cardiac dulness was slightly diminished; pulse, at the wrist, was small, weak, but regular, 80 in the minute; skin hot and dry, but neither it, or the conjunctivæ presented any jaundiced tinge.

Dr. Cryan said that when he first saw the case, he had some little doubt as to the nature of the hepatic enlargement—the intemperate habits of the patient, the firm, resistent, though smooth, liver felt below the right costal arch, the dyspeptic symptoms and the ascites, seemed to suggest that it might be due to cirrhosis in its first stage, in which, before the process of contraction sets in, we occasionally find the organ slightly enlarged, and firm and smooth; but, against this view were the absence of splenic tumour, of any enlargement of the superficial abdominal veins, or of gastric, or intestinal hemorrhage, and the marked cachexia, and rather rapid course of the disease; again, the smooth, painless enlargement of the organ might arise from waxy liver, but in waxy enlargement the progress of the disease is slow, and there is usually, enlargement of the spleen, albuminuria, with marked increase in the quantity of urine, and a history of constitutional syphilis, or of chronic disease of the bones,—all of which were absent in the case before us. That he (Dr. Cryan) therefore, thought it probable, notwithstanding the absence of pain, and the inability to feel any tubera projecting from its surface, the enlargement was due to cancer of the liver.

On the 30th of December—ten days after the patient's admission into hospital—two or three round firm nodules, each about the size of a marble, were distinctly felt on the anterior surface of the left lobe; the entire liver had, meantime, increased much in size; the emaciation continued to progress; the patient passed sleepless nights—though it is worthy of record that *he never complained of pain*—and the cachexia was more pronounced, so that there could be no longer any doubt as to the cancerous nature of the malady.

On the first of January, 1869, he became slightly jaundiced; the stools still contained an abundance of bile—the jaundice persisted till his death. On the 29th of January—ten days before the patient's death—the *right*

lower extremity became anasarcous, the swelling extended gradually up to the crest of the ilium; there was no tenderness over the veins of the limb, nor did the patient complain of pain; the skin was pale, and the temperature a few degrees below the normal standard; pulsation could be felt in the dorsal artery of the foot. From this date, until he died of exhaustion, on the 8th February, the œdema of the lower limb slowly increased.

Post mortem examination, 14 hours after death, exhibited the following appearances:—The liver was enlarged, weighing 5lbs. 4oz., and was thickly studded on both surfaces, and throughout its interior, with round whitish tubercles of brain-like consistence, and varying in size from a pea to a chesnut, but few of these tubercles presented the usual cup-like appearance on the free surface, or showed any central softening; the hepatic tissue surrounding them was for the most part of a yellowish brown colour, but at some points, where the tubera were of large size, the liver substance was of a greenish hue, and seemed condensed by their pressure; three or four large nodules projected from the anterior surface of the left lobe of the liver, close to its thin edge. On a microscopic examination of the cream-like fluid obtained by scraping a section of the tubercles, it was found to be made up of large nucleated, round, oval, and fusiform cells, ranging from the $\frac{1}{500}$ to $\frac{1}{1000}$ of an inch in diameter, with a nucleus about one-third the size of the cell, and surrounded with much fine granular matter, which resisted the action of ether, and was not of a fatty nature; some of these cells contained two nuclei; the hepatic lobules were not infiltrated with cancer, but many of the hepatic cells were loaded with oil, and some few looked withered and small; the hepatic cells in the centre of each lobule were nearly filled with bile pigment; the cancer cells appeared to have been, in many places, altogether confined to the interlobular connective tissue. The glands in the transverse fissure of the liver were infiltrated with cancer, and formed a tumour as large as a hen's egg, which embraced the common bile duct and the blood vessels. The trunk and primary branches of the vena porta, hepatic artery, and bile ducts, were all quite pervious, and free from coagula, and but little altered in size; the cystic duct, however, was much compressed, and superficially ulcerated at its origin from the gall-bladder, by one of these cancerous glands; the gall-bladder was contracted, its coats were thickened, and it contained about half an ounce of dark green bile. The head of the pancreas was slightly infiltrated with cancer, was hard and of a greyish-yellow colour, but the opening of the common bile duct into the duodenum was quite pervious. The right common iliac vein was much compressed by a large cancerous gland; its cavity did not contain any coagula, or cancerous masses; the iliac arteries were quite free and pervious. The stomach and intestines were normal; the spleen was small, weighing $4\frac{1}{2}$ oz., its fibrous coat thickened, the parenchyma firm and of a

dark brown colour. Iodine and sulphuric acid did not develope any so-called amyloid reaction when applied to the tissue of the spleen, liver, or kidneys; the latter were small, hard, smooth, and congested, weighing only 3½ oz. each; the heart was very small, weighing only 5½ oz., its muscular tissue was pale and softer than natural, but microscopic examination did not reveal any fatty degeneration; the cardiac valves and orifices were healthy; the lungs were much congested, but, in other respects, quite normal. Dr. Cryan stated that it was worthy of notice that in this case there was complete absence of all pain, thus affording a further illustration of the clinical fact that encephaloid disease of the liver is often painless, and in marked contrast, in that respect, with true scirrhus of the organ, which is comparatively rare, but an interesting case of which has been recorded by Dr. Stokes—*Proceedings of Pathological Society*, New Series, Vol. iii., p. 170,—in which "the principal symptoms were jaundice and *intolerable and persistent pain*." The great atrophy of the heart in the present case was also of interest, as well as the cause of obstruction to the circulation in the right common iliac vein revealed after death, and explaining the œdema of the right lower extremity noticed during the last ten days of the patient's life. In a case of cancer of the liver recorded by Dr. Gordon (*Proceedings of Pathological Society*, New Series, Vol. ii., p. 160), the patient suddenly complained of severe pain in the left lower extremity, rapidly followed by great swelling, and a fall of several degrees in its temperature, with absence of any tenderness on pressure over the large veins.—Dr. Gordon diagnosed that the main artery of the limb had been suddenly occluded, and *post mortem* examination verified the diagnosis, by showing that the interruption to the circulation was caused by the left common iliac artery being completely obstructed by a mass of semi-fluid cancerous matter.—*February* 20, 1869.

Rheumatic Inflammation of Pia Mater.—Dr. Little said, a fortnight ago a lad, aged eighteen, was brought into his ward in the Adelaide Hospital suffering from acute rheumatism. It was his third attack, he having had one four years, and another two years previously. He had been ill about a week before the date of his admission. It was a case of fibrous rheumatism, as distinguished from synovial rheumatism; that is, the joints were not swollen to any extent or painful, as long as he kept them perfectly at rest, but the least motion or manipulation caused severe pain. It was also that sort of rheumatism that moves from one joint to another. When admitted the left shoulder, wrist, knee, and ankle were attacked; whereas, for the first two or three days of his illness, he said it was the joints on the right side of the body alone that were affected. He presented the usual symptoms of rheumatic fever, except that the sweating was not so marked as it usually is; he had

retention of urine. On listening over the heart there was a distinct murmur to be heard, with the first sound most audible over the third and fourth left costal cartilages and the intervening space. During the week following admission the pains in the joints became less, the use of the catheter became no longer necessary to relieve the bladder, and some palpitation which he had about the heart ceased. On that day week, however, a change was noticed in his condition. On the previous evening the nurse said he was a little drowsy, and when Dr. Little saw him in the morning the drowsiness was very apparent. When roused up he appeared to be very weak; he fell over, and could not be kept awake for more than a second or two at a time. On examining the chest no alteration in the cardiac murmur was observed, except that it was not so distinct as before, probably because the heart's action was exceedingly rapid. Over the base of both lungs posteriorly there was dulness, particularly marked over the base of the left lung, where there was to be heard a friction sound, whiffing tubular breathing, and occasionally crepitus. The stupor became gradually more intense, and he died that night. On Monday last an examination of the body was made. On the thoracic cavity being opened about a pint and a-half of serum was found in the pleural sac; the lower lobe of the left lung was in a state of splenization; an ounce of serum was found in the sac of the pericardium, and in it floated one small shred of lymph. On opening the heart he found the murmur which had been observed was due to former disease. The appearances at the aortic opening were due to old disease, and doubtless had their origin in a previous attack, one segment of the valve being opaque, thickened, and corrugated.

In the ventricle was found a firm fibrinous clot, which presented evidence of having been formed during life, inasmuch as it was furrowed as if by the current of blood flowing into the aorta.

Thirty hours after death the blood flowed from the ascending vena cava with as much freedom as it would from a vein during life. It was alkaline, but not more so than blood should be; a quantity of it being placed in a glass it formed a thin small clot, not at all firm. The veins on the surface of the brain were unusually full of blood. On slicing the right hemisphere of the brain there was found in the lateral ventricle no departure from health, but on opening the left lateral ventricle they found three drachms of turbid serum, which, under the microscope, was seen to contain blood corpuscles and distinct lymph globules. Head symptoms coming on in cases of rheumatic fever were doubtless usually connected with inflammatory change in the heart, but occasionally, as in this case, they arose from the rheumatic poison attacking the structures within the skull. It was to be observed that it was the left lateral ventricle, corresponding to the side on which the joints were attacked, in which this turbid serum was found; and the finding of this turbid serum within the ventricle

was an illustration of the tendency of the rheumatic poison to attack certain structures. Fluid free in the cavity of the ventricle must have been derived from the pia mater, a fibro cellular membrane, and approaching therefore in structure to the tissues more commonly attacked.—*February* 20, 1869.

Abnormal Condition of the Red Blood Corpuscles.—Dr. Purser said:—I wish to bring under the notice of the Society, and so to place on record. a curious observation which I have recently made. On examining the blood of a frog into whose lymphatic spaces some milk had been injected about twenty-four hours previously, the white corpuscles were seen to contain, almost without exception, fat globules in variable number, some holding only one shining particle, while others were so gorged with oil drops as to resemble the colostrum corpuscles of milk. This was in no way surprising; but the red corpuscles also contained each one or more refracting grains, which at first sight closely resembled fat. On more careful examination, however, they were found to differ from fat in having a less refracting power, in being unacted on by ether while they dissolved slowly in distilled water, more readily in ammonia, and with great rapidity in acetic acid and caustic potash. They were hence of a proteinaceous nature. The greater number of corpuscles contained only two or three such particles, but as many as nine were counted in some. They lay about midway between the nucleus and periphery of the cell generally, were multiple in lines, but sometimes in small groups of three or more. The blood coagulated firmly, and did not differ in any other respect from healthy blood. A careful spectroscopic examination was made. The absorption bands were identical with those of normal hemoglobulin. I may mention that the frog died a short time after being made motionless with curara for the purpose of microscopic examination of the circulation in the mesentery. The blood was examined about four hours after death. I am quite unable to assign the import of this observation. That it was unconnected with the injection of milk I am convinced, for in many other cases in which I have performed this operation, although the white corpuscles were loaded with oil, the red corpuscles were unaltered. I have thought it worth recording, as I have never seen or heard of a similar case.—*March* 6.

Primary Cancer of the Liver.—Dr. John H. Benson said:—These specimens, Mr. President, were taken from the body of a man, aged seventy-one, who died under my care, in the City of Dublin Hospital, a few days ago. He stated that he had always been a healthy man up to about a year since, and that then he began to suffer from a sense of uneasiness and weight in the right side, which gradually increased up to

the date of his admission, but was at no time severe. He also stated that he had lived temperately for many years. However, I was told that he boasted to the other patients in the ward that he usually drank about nine glasses of whiskey before breakfast.

On admission, the patient complained of a "soreness," to use his own expression, "from the groin to the shoulder," and on examining the right side I found evidence of a very large liver. It extended downwards almost to the umbilicus. There was considerable tenderness over the whole liver, but it was especially marked in certain spots. On palpation a sense of hardness was communicated to the hand, but I found it impossible to say whether the surface of the liver was smooth or otherwise, owing to the tension of the recti muscles whenever pressure was made on the part. Notwithstanding this tenderness, there was a total absence of pyrexia; pulse 65, regular, but small; tongue slightly furred. No œdema existed anywhere, nor any acites. The urine contained a small quantity of albumen. Judging, then, from the absence of pyrexia, from the apparently slow development of the disease, from the history of his intemperate habits, and from the absence of signs of organic disease in any other viscus, I was induced to set the case down as one of either ordinary fatty, or of amyloid disease of the liver, and I treated it accordingly.

For nine days the patient progressed favourably, saying that he felt better from day to day. On last Monday, however, the fifth day from admission, he did not feel so well, and for the first time a slight jaundiced hue was perceptible over the body. On the next day the yellow colour of the surface was intensified; the urine was tinged with bile; pulse 104, small and irregular; respiration 25, deep and thoracic. He lay in a kind of torpor, but when spoken to he roused up and answered intelligently. This semi-comatose condition directed my attention to the kidneys, but I found that the urine was copious in quantity. On Wednesday all the symptoms were seen to have become considerably aggravated, and in the evening he sank and died.

On opening the abdomen, assisted by my colleague, Dr. Hewitt, we found from about four to six ounces of fluid, containing a large proportion of blood, in the cavity of the peritoneum. There was no evidence of peritonitis. Numerous petechial extravasations of blood, varying from one to three or four lines in diameter, were visible in the mesentery. The liver was found enlarged to twice its normal size, weighing 8 lbs. 5½ oz. The substance of the liver in parts presented somewhat the appearance of the amyloid disease; but, on applying the iodine and sulphuric acid test, no characteristic reaction took place. The greater portion of the liver, however, was of a purplish red hue. Besides this alteration, we discovered numerous tumours, varying from the size of a hazel-nut to that of a small orange. They were invariably raised above

the surrounding surface, and with a very few exceptions were of a much darker colour, and somewhat softer and more elastic to the touch than the surrounding structure. They were not depressed in the centre. When cut into, they presented a circular or oval section, with a well-defined margin, and their substance projected with a slight convexity, about equal in degree to that of a watch-glass. With few exceptions the diameters of the tumours when cut into were found to be considerably greater than that of their disc which appeared on the peritoneal surface of the liver. Dr. John Barker kindly examined one of these tumours, and pronounced it to be medullary cancer. On raising the liver, we found that the lymphatic glands in the transverse fissure were greatly enlarged, and were so soft that a slight pressure of the fingers reduced them to a soft grumous pap.

Suspecting these tumours to be cancerous at the time we discovered them, we next proceeded to look for cancer elsewhere. The stomach contained some dark, altered blood, but no cancer existed there. The intestines and the other abdominal organs were equally free. On opening the thorax, we discovered a number of enlarged glands on the right side exclusively of the anterior mediastinum.

In the apex of each lung we found a few old tubercular masses, which had undergone calcareous degeneration, and a very few small old cavities, but no trace of cancer was perceptible. However, while opening the pericardium, we discovered a tumour, about the size of a walnut, situated in the angle formed by the junction of the pericardium and diaphragm. This tumour I examined myself, and found it to be cancerous. A few enlarged glands were found in the right side of the neck. No other traces of cancer, besides those I have mentioned, were visible anywhere in the body.

The heart had undergone some fatty degeneration. A few calcifications were imbedded in the substance of some of the chordæ tendineæ of the left side, and in the left zona tendinosa. On the free edge of two of the tricuspid flaps we found a somewhat bulbous smooth swelling, yet these gave rise to no murmur during life.

The kidneys presented appearances due to Bright's disease, and in some places may be seen that appearance of the pyramids said to resemble a sheaf of wheat. On one of the kidneys we saw a large cyst, such as we sometimes find in cases of Bright's disease.

This case, Sir, is interesting, I think, as presenting an example of primary cancer of the liver, which we know to be rare, and as not exhibiting many of the most important diagnostic symptoms of that disease. The entire absence of cachexia, or of pyrexia, and the apparently slow progress of the disease, the suddenness of his death, and the absence of cancerous disease in other organs, are remarkable. The age of the patient, too, was somewhat unusual. Out of eighty-three cases

analysed by Frerichs with reference to age, only two exceeded seventy. This patient's age was seventy-one.—*March* 6, 1869.

Malformation of the Chordæ Tendineæ ; Congenital Musical Bruit.—Dr. Mayne said the specimen to which he wished to call the attention of the Pathological Society was one of considerable interest. It was taken from the body of a man named Edward Kelly, aged 69 years, who died some weeks before in the hospital of the South Dublin Union, under the care of his friend, Dr. Jennings, to whom he was indebted for permission to exhibit it here to-day. The case, he considered an interesting one, in two points of view, as affording an explanation of an exceedingly rare cardiac bruit ; and as also indicating the connexion which, he believed, frequently existed between obstructive cardiac disease and derangement of the cerebral functions.

In the year 1860, at which time his father, the late Dr. Mayne, was physician to the above institution, Kelly first came under medical notice. He read a very brief entry, made by Dr. Mayne in that year "March 24th.—A musical systolic bruit, most intense at the left apex, inaudible between the spine and scapula, pulse regular and full."

For a considerable period after this Kelly continued in good health, but subsequently he became subject to epileptiform convulsions, which occurred every three months, and occasionally to fits of mania, during which he was very violent and unmanageable. He had the well-marked symptoms of aortic disease for the last three or four years of his life, and he finally sank under the complications arising from the latter disorder.

The musical bruit first demanded attention. If the interior of the left ventricle were examined, a ready solution of the difficulty would be discovered. Two irregular chordæ tendineæ would be observed, which stretched across the area of that chamber, one from the posterior wall to the zona tendinosa, which surrounded the aortic opening, and the other from the former to the anterior wall.

Those present who were familiar with the anatomy of the heart, would at once perceive that this arrangement was most irregular, as the chordæ tendineæ normally extended from the musculi papillares on the sides of the ventricle, to the corresponding divisions of the mitral valve, and in no instance which he could discover had they been known either to cross that chamber, or to be attached to the aortic opening.

He had no hesitation in stating that the blood in its passage over the first of these cords produced the strange musical note ; and he believed it was totally unconnected with the condition of the aortic valves, which had undergone, as might be perceived, osseous degeneration, and had converted the aortic opening into a mere chink.

The bruit was unquestionably congenital. It was remarkable also in being *systolic*, and in being most intense at the apex.

In the other recorded cases with which he was familiar, the sound was diastolic, and was most audible at the base. The attachment, however, of this irregular cord would easily explain the direction of its intensity.

Dr. Banks, in the 4th Vol. of the *Dublin Hospital Gazette*, described a case of musical bruit, where the sound, a "single regurgitant one," was most audible at the base, and was produced by three rudimentary valves, each placed about $\frac{1}{4}$ of an inch below the aortic opening, and attached to the sides of the ventricle. These occupied pretty much the same position as the irregular cord in the heart exhibited. Dr. Stokes mentioned an instance where a similar sound was produced by an osseous infiltration of the commencement of the aorta. Sir D. Corrigan knew a tongue of bone $\frac{1}{4}$ of an inch in length, projecting into the same vessel to elicit a like phenomenon.

Some present might regard the disease of the aortic valves as the true cause of the bruit, but he had been influenced to form a different opinion from the following considerations:—

1st. That the sound existed years before there were any other signs of aortic disease.

2ndly. That it was utterly impossible for the blood to escape from the ventricle into the aorta without producing a vibration of the tense cord stretched across that aperture, which must have been accompanied by a sound of more or less intensity.

3rdly. That the sound was most audible at the left apex, exactly corresponding to the inferior attachment of this irregular cord.

He was inclined to attribute to the extreme obstruction of the aortic orifice, and the consequent deficient vascular supply of the brain, the epileptiform convulsions, and maniacal seizures with which this case was complicated.

He thought these sequelæ of cardiac affections had not as yet been sufficiently recognized by the profession.—*March* 6, 1869.

Hypertrophy of the Heart.—Dr. Grimshaw exhibited a case of an enormously large heart taken from the body of a patient who had been under the care of Dr. Burke in Steevens' Hospital. The man was forty years of age, and a bricklayer. He had been ill twelve months before admission to hospital. He gradually became worse, and was admitted on the 19th of February. The chief point of interest was the enormous pulsation of the heart. The whole chest seemed to move in front with the force of the pulsation. There was great dyspnea and some œdema of the lower extremities. He had never had rheumatic fever. There was of course an enormously increased extent of precordial dulness, and Dr. Burke detected a distinct murmur with the second sound of the heart. The patient died on the 21st of February. On a *post mortem* examination

all the cavities of the heart were found enormously thickened. He had seldom seen them so thick. The only valvular disease to be detected was that of the aortic orifice. The valves were extensively diseased, and the aorta enormously dilated.—*March* 6, 1869.

Hypertrophy of the Heart.—Dr. MacSwiney said—Four weeks ago, a man between sixty and seventy years of age, applied to me for medical advice. He complained of extreme distress of breathing, great palpitation of his heart, and a distressing cough. I admitted him to Jervis-street Hospital, and upon instituting a careful examination of his chest, I found the area of precordial dulness considerably increased, and I also found a perceptibly increased cardiac impulse. He had irregular pulse, and plain evidence of bronchitis in both lungs. He was put under treatment which did not materially improve his condition. On the contrary, he gradually became weaker, and ultimately died. One week before he died he expectorated large masses of black blood. Perhaps half a dozen or a dozen of these sputa would be expectorated in the day, and corresponding with the symtoms I have described there appeared at the base of the right lung—rapidly extending upwards—dulness on percussion. He died asphyxiated, or, strictly speaking, by apnea. The parts which I now exhibit consist of the lungs and the heart. The left lung was to all seeming healthy. The right was the subject of most extensive pulmonary apoplexy, of that character where the pulmonary tissue is infiltrated throughout with blood. The heart was found to be very considerably hypertrophied, and the hypertrophy was confined exclusively to the left ventricle. It was simple hypertrophy, being neither eccentric nor concentric in any remarkable degree, but being an increase of the muscular structure of the fibres of the left ventricle, without any contraction or dilatation of that chamber. I omitted to remark that there was no abnormal bruit audible during life in the precordial region. The sounds were perfectly normal on the several occasions I examined him; notwithstanding this, I confess I expected to find evidence of valvular disease. None such, however, existed. The aortic valves were perfectly sufficient, and quite competent to retain a column of water when poured into them, as may now be seen. The mitral valves were healthy in every particular. The auriculo-ventricular opening was normal. The kidneys were normal in every respect, save that they were the subject of very considerable sanguineous congestion. Similarly the liver exhibited no trace of disease cognizable by the senses. When cut into, it also gave vent to a great mass of black blood. This was explained by the circumstances that attended the form of death in this instance; it being by apnea it was to be expected that the right chambers of the heart would be occupied with black blood, and all the organs essential to life congested with dark blood.

To those who specially devote their attention to cardiac and pulmonary

diseases there are two features of considerable interest in this case. Firstly, there is the occurrence of pulmonary apoplexy without any valvular disease of the heart. Perhaps it will be found on investigation that instances in which pulmonary apoplexy has been recorded to have existed, are extremely rare in which there was no mitral disease. The second point of interest is the existence of this remarkable hypertrophy of the wall of the left ventricle, more than an inch or an inch and a half in thickness, without a trace of aortic insufficiency or other valvular disease. It is known that the existence of aortic valve disease is the most common cause of the occurrence of this lesion; and I am not aware that there are many instances recorded where it has been found present, and where no organ of the body exhibited any evidence of structural disease. Anything which causes constant excitement of the heart's action is competent to cause enlargement of the muscular structure of the heart. Now, this man was an habitual drunkard for thirty or forty years, and the hypertrophy may be to some extent due to constant intemperance.

It is proper to remark, however, that when the aorta was split open, it showed evidence of atheromatous disease but trifling in amount. Now we know that the presence of atheroma affecting the vessels is capable of interfering with their elasticity, and anything capable of interfering with the elasticity of the blood vessels is capable of producing hypertrophy of the left ventricle in a degree proportionate to the extent and duration of the lesion of vascular tissue.—*March* 13, 1869.

Wound of the Heart.—Dr. Fleming exhibited the heart of a soldier in the Royal Artillery, who had committed suicide by stabbing himself a few days previously. It appeared that the man had been intemperate, and, owing to his excesses, shortly before his suicide, he had become completely unmanned. He had been heard to speak of the uneasiness which he experienced, and also of putting an end to his existence. On the occasion of one of these attacks of intemperance he was asked to go to his room, which was adjoining the apartment in which he was at the time. He said he dreaded the idea of going to bed, that his sensations were frightful, and he even at that time mentioned that he would put an end to himself. He spoke to the wife of the sergeant, and she suddenly heard a click like that of a penknife. She seized his arm, he pulled the knife forcibly out of his body, laid it deliberately on the table, and said, "Now I should like to be placed where I can die with ease." Ten minutes afterwards he was dead. Dr. Cuff saw him very soon after the occurrence; he was then pulseless, in a state of complete collapse, able to lie horizontally, and inclined to the right side. His breathing, as described by Dr. Cuff, was suspirious; he would draw two or three breaths, then cease, and after irregular intervals resume the act of inspiration.

Along with the heart, he now exhibited a small portion of the integument over the apex, in order that they might see the very slight wound that had reached and penetrated the heart. There was hardly any external bleeding. The amount of hemorrhage stained the shirt in the neighbourhood of the wound, but not more than was sufficient to attract attention, and no discharge of blood took place subsequently. The depth to which the knife had penetrated was nearly three inches, and it was supposed he took the knife in his left hand, suddenly opened and drove it into the heart. It was found that he had perforated the heart at its apex, the knife having been driven through the left ventricle. The pericardium was distended with blood, partly fluid, and partly coagulated. The pleura also contained a great quantity of blood. There was an enormous amount of fat deposited on the heart, but there was no fatty degeneration.—*March* 20, 1869.

Cystic Tumour of the Breasts.—Dr. Robert M'Donnell exhibited to the Society a tumour, weighing about two pounds, which he had removed on November 23. It was, in fact, a recurrence of the disease in the patient whose case he had brought under the notice of the Society a year ago (5th December, 1868).

It was unnecessary to refer to the early history of the case, as this was already detailed in the records of the Society; suffice it to say, that the original tumour had been a very large sarcomatous tumour of the breast, of the variety known as myxoma. It had recurred, as these growths unfortunately often do, and for six weeks previous to the second operation had increased rapidly in size. There was no reason to change the opinion formed as to the nature of the disease; although recurrent, there was no more evidence of its being malignant, in the true sense of the word, than at the time of the first operation. The patient's general health was excellent, and there was not any trace of glandular enlargement in the axilla. The rapidity of its growth during the last few weeks made its removal necessary.

The structure of the recurrent growth was identical with that of the original tumour. No cysts. however, had formed, but the soft "colloid" structure of the mass yielded a larger quantity of gummy juice; a small portion of the tumour, when bruised up in a mortar with silver sand, and treated with cold water, yielded on filtration a fluid thick as flax-seed tea, and containing abundance of mucin, the presence of which Dr. M'Donnell demonstrated to the members present by its peculiar reaction on the addition of acetic acid.—*November* 27, 1869.

Suppuration of the Eyeball after Injury.—Mr. Wilson exhibited an eye removed from a mechanic, who, two years previously, received an injury

of it from a piece of metal, in consequence of which the eye became blind. About the 1st October this blind eye was again injured by hot metal striking it, in consequence of which the organ became exceedingly swollen and painful, and the man was unable to obtain any rest or sleep for a long time. When Mr. Wilson saw him, five weeks subsequently, there was considerable chemosis and pain, and the vision of the other eye was somewhat impaired; slight lachrymation and intolerance of light were also present in this eye.

To save the uninjured eye, as well as to relieve the great suffering, Mr. Wilson removed the eyeball, and on opening it found the space, recently occupied by the vitreous humour, filled with pus, the retina detached and funnel-like, attached behind only to the optic nerve, and in front to the ciliary region. The space between the retina and the choroid was filled with a sanguineous fluid; the choroid itself was healthy. There was a large mass of firm neoplastic tissue at the ciliary region, which would account, in a great measure, for the pain and suffering, as well as for the sympathetic affection of the other eye.—*November* 27, 1869.

Rupture of the Vena Cava.—Mr. Wharton exhibited an example of rupture of the vena cava, close to the diaphragm, together with a rupture of the small intestine, caused by the passage of the wheel of a car over the abdomen. The patient was a child, aged twelve, and was brought to the Meath Hospital at once. The injury was pronounced to be a ruptured intestine. There was great and constant desire to pass water, but when a catheter was introduced, not more than half an ounce of urine flowed through it. The abdomen, in appearance, somewhat resembled that observed in cases of distended bladder. There was no fracture or mark of violence externally. Death took place at the end of twenty hours after the receipt of the injury. The quantity of blood effused was not very considerable, in consequence, most probably, of the syncope which ensued, the small size of the rent in the vessel, and the formation of a clot at the aperture. The largest quantity of blood was found under the right kidney. The peritoneal cavity contained a large quantity of serum. Mr. Wharton observed that Professor R. W. Smith had, many years ago, reported a case, in which the superior vena cava had been torn completely from the right auricle in a woman, whose chest was violently crushed between a heavily loaded cart and a wall, but there was no fracture of any part of the osseous walls of the thorax.—*November* 27, 1869.

Fracture of the Ribs.—Mr. Hamilton gave the following history of this case:—

A healthy man, twenty-five years of age, fell from a scaffold twelve feet high. He was brought to the Richmond Hospital in a complete

state of collapse. On examination, it was found the left clavicle was broken, and the fragments so sharp that they had almost perforated the skin. The ribs were broken on the left side, and driven in so that the hand could be passed into the depression. There was a good deal of difficulty of breathing, and a constant spitting of blood. During the night the hemoptysis continued, the blood being partly fluid, and partly clotted. He passed also bloody urine. He went on pretty well until the third day. On the night of that day he became affected with a most dreadful pain in the region of the left kidney, and after a time he passed a very large quantity of dark fluid blood. For some few days afterwards there was considerable pain in the region of the kidney, but it subsided, and the bloody urine and the hemoptysis ceased. The pulse came down to 80, and the man showed signs of decided improvement. After the lapse of a fortnight, having got up to go to the night chair, he suddenly became quite pallid, was bathed in a profuse perspiration, his lips became perfectly livid, and he died before any assistance could be given him.

The following were the results of the *post mortem* examination: There was a comminuted fracture of the clavicle, with overlapping of the fragment. Seven ribs on the left side were found to have been broken near their posterior angles. They were very fairly in apposition, and there were no spiculæ. The left lung had become closely adherent to the ribs by lymph, effused along the line of the fractures, and in the vicinity. In two places the broken ribs had perforated the surface of the lung, but the perforations were exceedingly small, and scarcely sufficient to account for the great hemoptysis which had occurred during the progress of the case. When the hand was placed on the lung therewas found to be a substance in it almost as large as an orange, and when the lung was cut across, it was found that this was a firm coagulum of blood which filled a rent of the upper lobe. In the lower lobe, also, there was a laceration of the pulmonary tissue, and the lung was congested with blood. This state of the lung, it was obvious, arose from the great violence with which the man had struck his side in the fall, breaking seven ribs, and crushing the lung. He apprehended it was one of the original vessels that had broke out from the lung in the first instance that had given way, and this was rendered the more likely by what occurred, for the man smoked a pipe, got up, and went to the night chair, when, to use his own words, he felt as if his heart-strings gave way, and a quantity of blood poured out. During life the man had passed bloody urine, and complained of the kidney on the left side, and it was easy to diagnose that the kidney had been crushed by striking against some projecting part of the scaffold. They found in the cellular tissue around the kidney a quantity of extravasated blood. The interior of the organ presented a similar condition, owing to the rupture of its vessels by the violence of the contusion.—*December* 4, 1869.

Hepatic Abscess.—Mr. H. G. Croly said the specimen which he now laid before the Society was one of abscess of the liver, removed from the body of a man, aged forty-one, who had been admitted to the City of Dublin Hospital, on the 5th of November, suffering from phthisis. He stated that he had been first attacked with illness two years back. On admission he was greatly wasted and emaciated, but till the day previous to his death, he declared he felt himself improving and getting stronger. It was not until the day before his death he complained of a serious change. He died on the 24th of November. His chief symptoms were wasting, night sweats, cough, and a certain amount of pain under the right clavicle. They could not discover any extensive disease in the lungs, but looked on the case as one of scattered tubercles. He died suddenly. Half an hour before his death Dr. Croly saw him. He got up and dressed himself, when he suddenly fainted, and then became covered with a cold clammy sweat. Restoratives were exhibited, but they failed to produce any effect. He never rallied, and soon expired.

Upon laying open the chest its contents were found to be healthy, with the exception of a few tubercles in the upper part of the right lung. On opening the abdomen to obtain the liver for exhibition (for during life they had examined this organ frequently, and believed it to be an example of the fatty liver of phthisis), a large abscess suddenly burst, which had occupied the entire of the right lobe. The matter dicharged from this abscess filled the abdominal cavity in a few seconds. It was extremely fetid, and contained a number of large flakes. It presented all the appearance of strumous matter coming from a large chronic abscess. He supposed this abscess gave way suddenly, but he could not say whether it had given way before he had raised the liver out of its position, or during the separation of the liver from the diaphragm, to which the upper portion was adherent. It was important to remark, that the man had no diarrhea at any time, or anything to lead them to suppose he had ulceration of the bowels.—*December* 4, 1869.

Suicidal Fracture of the Larynx.—Mr. W. Stokes, jun., laid before the Society a specimen showing a comminuted fracture of the thyroid cartilage. On the 20th of July last, a man was brought into the Richmond Hospital suffering from a wound in the throat, which he had inflicted on himself with a large rusty bread knife, causing a gaping chasm, so wide that they could get a complete view of the vocal apparatus; the true and false vocal chords could be distinctly seen moving during respiration and attempts to speak. They could also, on looking upwards, see the epiglottis rising and falling during the acts of respiration. When the patient had his head thrown backwards and attempted to speak, the articulation was extremely indistinct, but there was this remarkable phenomenon connected with it—a double voice—one voice appeared to

come through the wound, and the other through the mouth, each differing in character. When the head was thrown forwards, the articulation became distinct. There was no hemorrhage, although there must have been a great deal before he came into hospital, for his clothes were saturated with blood. The patient stated that he had been a man of intemperate habits for years, sometimes drinking for weeks together without intermission, and he suffered from great depression of spirits, and had many times thought of taking away his life. He had, in fact, made two previous attempts to do so. Once at Greenock he threw himself off the pier into the river; and on the second occasion he attempted to cut his throat; the third and successful attempt was the result of a sudden impulse. He had no idea of it, he said, a few moments before inflicting the wound. It was an instantaneous action, and done without a moment's reflection. Everything that was done for the man proved to be quite unavailing, owing to his very restless condition. On the evening of the second day he got out of bed, and roamed about the house, and the patients thought he was seeking for some instrument to complete the work he had begun. For the third and fourth days the patient appeared to be going on better, but on the fifth day his breathing became stertorous, and he seemed to be sinking. Under suitable treatment he rallied from this state, but on the afternoon of the 6th day he became worse; his pulse was 70, extremely weak and irregular; respiration became very rapid, and his mind wandered.

The following note was taken by the clinical clerk at 9.30 p.m.:— "Auscultation reveals a sound above the chest like to that of a car moving at a distance. The inspirations are almost equal in number to the heart's impulse, viz., 76 to 80 in the minute. He says he is suffering great pain all over the chest, and in the region of the liver and stomach."

Such were the principal points connected with the case. It was one of interest, as having afforded an opportunity of viewing the entire of the vocal apparatus better than could be obtained by any laryngoscope, and as showing the great length of time a patient could live after the infliction of so formidable an injury. The double voice, the peculiar respiratory sound, the rapidity of inspiration, and the absence of any pulmonary congestion or inflammation (notwithstanding the exposure to which the patient subjected himself by wandering about the hospital), were also a feature of interest and importance.

The wound passed immediately before the superior margin of the thyroid cartilage, and the man must have made several gashes with the knife, for the thyroid cartilage (partly cut and partly broken) was divided into seven pieces. The posterior wall of the larynx was opened into the pharynx, showing the true and false vocal chords.—*December* 4, 1869.

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16. The Practitioner; a Monthly Journal of Therapeutics. Macmillan and Co.
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INDIA.

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AUSTRALIA.

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AMERICA.

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34. The Western Journal of Medicine. Indianapolis, Ind., U.S.A.: C. D. Cazenove, London.

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36. Gazette Hebdomadaire de Médecine et de Chirurgie. Paris: Victor Masson.
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75. Norsk Magazin, for Lægevidenskaben. Udgivet af et medicinske Selskab i Christiania. Redigeret af W. Boeck. A. W. Münster. Lund: Voss. Christiania: Feilberg and Landmark.

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80. Giornale Veneto di Scienze Mediche.

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